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In Memoriam

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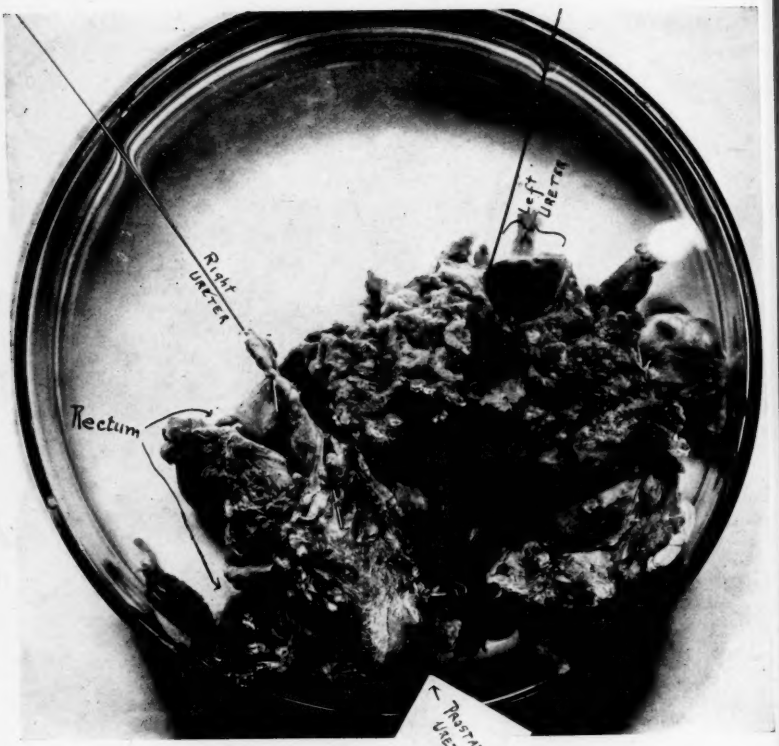
TRANSACTIONS

PLATE I.—Presented by DR. HORACE BINNEY



Gross specimen showing sarcomatous growth lining cavity of bladder and diverticulum

The American Association of Genito-Urinary Surgeons

First Day, May 15th, 1914

A CASE OF SARCOMA OF THE BLADDER

By Horace Binney, M. D., of Boston

THE comparative rarity of this form of bladder tumor and the unusual genito-urinary history of the patient lead me to report this case. The history is as follows:

J. T., 74 years, entered the Boston City Hospital, Nov., 1913.

Past History: About twenty years ago he was troubled with frequency of micturition. After a period of several years, during the last four of which he used the catheter once daily, he consulted Dr. A. T. Cabot, who found a considerably enlarged, rather soft prostate and a residual of twenty ounces.

In May, 1908, the prostate was removed by the perineal route. The pathologist's report was glandular hyperplasia. The result of the operation was unsatisfactory in that the bladder failed to empty completely, a residual of at least 8 ounces always remaining.

Seven weeks after the operation, cystoscopy showed a much trabeculated bladder wall, a small diverticulum in the lower left portion of the bladder, and a posterior bar, evidently the cause of some obstruction.

Dr. Cabot therefore operated again by the suprapubic route, cutting out the bar with prostatic forceps, which left a free opening between bladder base and prostatic urethra. The opening of the diverticulum seen with the cystoscope was too small to be noticed at this operation, and no other abnormality was observed at that time.

Again, patient and surgeon were disappointed by the failure of the bladder to recover its power of emptying, a residual of seven ounces being present a month after operation. The patient was therefore obliged to continue his life of partial dependence on the catheter, and reported 3 years later that he used it twice a day, drawing twelve ounces, as a rule, and that there was persistent cystitis.

The case was reported by me in June, 1911, before this Society as one of "Bladder Atony Following Prostatectomy."

During the following 5 years, the patient was in fair health, but gradually the urine became foul, loaded with mucopus, making it difficult to completely empty the bladder through the catheter in spite of irrigations twice daily.

In November, 1913, I found him weak, emaciated, with poor appetite, passing only 1 or 2 ounces of urine at a time, and suffering from almost constant desire to urinate because of the above difficulty with the catheter. This condition had existed for several weeks. His tongue was coated, skin dry, the urine ammoniacal with heavy sediment of pus and phosphates, the picture of pyelonephritis and foul cystitis. I therefore advised permanent suprapubic drainage, for which he entered the hospital.

Cystoscopy was attempted, but resulted in failure on account of the turbidity of the urine and fairly free bleeding.

On account of the renal condition on Dec. 1st, 1913, suprapubic cystotomy was performed under local anæthesia with novocain, and on opening the bladder I received a double surprise. First, the shape of the bladder had changed entirely since the previous suprapubic operation. My finger, on entering the bladder, at once met a ridge extending from

PLATE II.—Presented by DR. HORACE BINNEY

Fig 2.—Tumor cells infiltrating fat tissues
Magnified 120 x

Fig 1.—Mitotic figure in tumor. Magnified 1000 x

the anterior wall of the bladder backward and to the right. This proved to be the lower margin of an opening between the bladder proper and a large diverticulum, occupying the left side of the bladder, the upper margin of the opening being close to the suprapubic wound. The second surprise was my finding the walls of both diverticulum and bladder proper covered with soft, spongy, friable masses of pinkish-gray color, easily detachable and bleeding quite freely from the torn surface.

The extent of the disease and the patient's condition made any further operation unwise, and a large drainage tube was fastened in, with the hope of making the patient's remaining days more comfortable.

This slight operation was enough to turn the scale, and after 3 days he died of uræmia.

A specimen of the bladder growth, removed at operation, was examined by Dr. Mallory and pronounced sarcoma.

The autopsy findings are of special interest:

Peritoneal Cavity: Left side of pelvis obliterated by adhesions.

Kidneys: Large and soft. Pelves show a roughened, black, necrotic exudate on surface.

Ureters: Dilated uniformly and admit index finger. Lining presents same appearance as kidney pelvis. Lower end of each ureter, where it enters the bladder, admits a fine probe.

Bladder: Large, walls thickened, contains thick, black, foul-smelling necrotic material. The internal surface is very irregular, and contains cauliflower-like projections, which are pale, soft, and friable. There are some very firm nodules projecting into the cavity. On section the bladder wall is much thickened but very soft.

Urethra: The prostatic portion of the urethra contains a few small, injected nodules on its anterior surface. There is very little prostatic tissue.

Anatomical Diagnosis: Sarcoma of bladder, cystitis, ureteritis, pyelitis, sclerosis of kidney.

Cultures: Smear from bladder shows streptococci.

Microscopic Examination: Showed extensive atheroma of abdominal aorta, arteriosclerotic nephritis, acute ascending infectious nephritis, diphtheritic ureteritis, acute prostatitis and urethritis.

Bladder: The tumor is composed of spindle-shaped cells arranged in bundles running in all directions. The presence of occasional mitotic figures shows that it is growing moderately rapidly. The tumor has extended entirely through the bladder wall and is invading extensively the surrounding fat tissue.

As to the exact nature of the tumor, the pathologist states: "The diagnosis of the exact nature of the tumor cells is not possible because the tissue is not fresh enough to permit the use of differential stains for the demonstration of characteristic fibrils. The deep staining of the cytoplasm of the cells, however, strongly suggests that the tumor is a leiomyosarcoma rather than a fibrosarcoma."

Certain points in the clinical history of the case are of particular interest.

The thorough exploration of the bladder cavity made at the suprapubic operation in 1908 showed no sign of sarcoma developing at that time. The exact time during the next 5 years when the tumor began to develop cannot be stated because the patient noted no change in his condition until a few weeks previous to my seeing him in Nov., 1913, and no urinary or other examination was made during these 5 years. The comparatively recent change for the worse, and the very extensive involvement of the bladder wall found at operation suggest that the growth of the tumor was a rapid one. This is in accord with the views of Munwes,[1] who, in 1910, published an article bringing the subject well up to date, and based on a very thorough study of the literature. He found

PLATE III.—Presented by Dr. Horace Binney

Tumor cells. Magnified 375 x

in the majority of cases evidence of duration of the growth for less than one year before the diagnosis was made.

As to the etiology, the facts in this case seem to support the theory of chronic irritation as an explanation of the tumor's origin rather than that of the awakening of a foetal inclusion (Cohnheim's theory) which is the one advocated by Wilms. Moreover, the atonic condition of the bladder musculature—so marked that with the slight (if any) obstruction present there developed a large pocket or diverticulum of the left side of the bladder—would point to a lessened resisting power of the tissue cells.

That chronic irritation is a factor in the development of tumors of the connective or muscle tissue, as well as of the epithelial group, seems true from the case reports of several authors. Munwes found symptoms of chronic cystitis in a number of cases, persisting for a considerable time before the onset of more definite symptoms such as hæmaturia or difficulty in urination. Rehn,[2] who examined 45 workers with anilin dyes, found 3 tumors, one of which proved to be a sarcoma.

A detailed exposition of the subject of bladder sarcoma would be superfluous here, but it may be of interest to compare the frequency of the occurrence of sarcoma with that of other bladder tumors. Albarran[3] reported 2 sarcomata in 89 bladder tumors examined by him. Watson[4] gives a table of 134 tumors in which there are 121 epithelial tumors, 3 myomata, and 2 sarcomata, patients of all ages being included. Steinmetz[5] found in 32 bladder tumors occurring in children, 13 sarcomata. Munwes found, out of 719 cases reported in the literature, 4½% to be primary sarcoma. This is probably the most accurate figure.

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[1]Munwes: Zeitschrift für Urologie, 1910, Vol. IV, p. 837.
[2]Rehn: Archiv. für Klm. Chiv., Vol. L, p. 588.
[3]Albarran: Tumeurs de la Vessie, 1891.
[4]Watson & Cunningham: Genito-Urinary Diseases.
[5]Steinmentz: Deutsche Zeitschrift für Chirurgie, 1894, Vol. 139.

DISCUSSION

Dr. A. L. Chute, of Boston: I should like to mention a case of sarcoma of the bladder that I saw five years ago in a man of seventy-three. At the time of operation I supposed it was sessile carcinoma since there was nothing characteristic about the growth. The microscopic examination proved it to be sarcoma. One thing out of the ordinary was the fact that it recurred extremely fast. The patient made a perfectly good primary convalescence, but the growth recurred in a very short time and he died.

Dr. Francis R. Hagner, of Washington: I have had two cases of sarcoma of the bladder. One I am sure about; from the history of the other one, it does not look like it.

The first case was a man with a history similar to Dr. Chute's case. The growth was on the right wall of the bladder. It was removed, and he made a good recovery, but was dead within less than four months. There were evidences on cystoscopic examination of a return of the growth before he left the hospital. The pathological examination showed it to be a small round celled sarcoma. It was almost encapsulated. Through the cystoscope it looked smooth, and was not ulcerated. It had a very thin capsule around it.

The other case was a man who had had very frequent urination, and had been passing masses of the tumor. He was operated on and this tumor was removed, and the report came back that it was a small round celled sarcoma. He immediately had a recurrence. That has been now about seven years ago. That man is fortunate or unfortunate in having a lady doctor for a wife, and she won't let anybody see him. She says she won't have any more experiments done. He is still alive after seven years. He has a suprapubic drain in the bladder, and is still passing tumor masses. Several men in Washington and the pathologist at the Army Hospital there saw this tumor and all said it was a round celled sarcoma.

Dr. C. L. Gibson, of New York: In 1895 I removed a tumor of the bladder, size of a horse chestnut, on the supposition that it was a sarcoma. The excision was generous, convalescence was normal, and the patient never had any recurrence so long as she was kept under observation, some five or six years. Pathological diagnosis was never clearly settled. The tumor lay in the bladder wall covered by perfectly normal

mucous membrane. It was composed of small round cells. My own belief has always been, from the clinical manifestations, that we had a well-defined and very early case of sarcoma. This is, however, another instance of the unsatisfactory possibilities of the histological diagnosis of sarcoma.

Dr. Louis E. Schmidt, of Chicago: I have repeatedly referred to a case that I operated on several years ago of sarcoma of the bladder, in a young man, twenty-five years old, with an enormous tumor, extending way up to the umbilicus, and absolutely no capacity whatsoever. The entire bladder was removed except the trigone. It was an intraperitoneal operation. I might say some of the glands were even removed that were along the iliac vessels. I want to report that he is still alive, because two or three weeks ago I operated him for an inguinal hernia which came on recently. It is now ten years since the original operation.

Dr. Gibson: I would like to ask both Dr. Schmidt and Dr. Hagner if their cases were treated with the Coley toxins.

Dr. Schmidt: No.

Dr. Hagner: No.

Dr. Hugh Cabot, of Boston: I would like to ask Dr. Schmidt if there was a careful pathological examination made in his case.

Dr. Schmidt: Prof. Herzog, who is one of our able pathologists in Chicago, and several surgeons in Chicago, examined the specimen and were present at time of the operation. The report of the microscopic examination made by Prof. Herzog showed it to be a spindle celled sarcoma.

Dr. George S. Whiteside, of Portland, Ore.: I operated on a case of sarcoma of the bladder in a woman about sixty years old. I did not see this case early. I saw it in consultation in the last stages of the disease. The woman was passing masses of tissue and a great deal of blood at the time of the operation. The operation consisted only in looking in and giving it up. The entire bladder was full of spongy tissue, and the specimen, which I have, looked very much like the photograph which Dr. Binney has shown. The pathological report, made by the Pathologist of the University of Oregon Medical School, was that it was a spindle celled sarcoma, involving not only the bladder wall, but also the perivesical tissues. The kidneys, however, had not suffered much from the performance. The specimen that I have is a post mortem specimen. She died about two months after the operation.

Dr. Binney: Dr. Mallory went through the specimens carefully, and he was quite positive that there was no evidence of epithelioma in this case. I am sorry that the autopsy specimen was not more carefully removed, as it does not show the anatomy as well as it should.

INCRUSTED CYSTITIS

By John R. Caulk, A. M., M. D., of Saint Louis, Mo.

THE idea is quite firmly established that a cystitis which does not respond to ordinary routine treatment within a few weeks is usually tuberculous. There is, however, a type of cystitis which is equally as rebellious to the usual measures for relief as is tuberculous cystitis, and to which but a few pages are devoted in the mass of literature that has appeared on this subject. I speak of "Incrusted Cystitis" occurring in an alkaline urine. Realizing the extreme importance, and being impressed with the great seriousness of this malady, I am bringing it before you for several reasons: To emphasize its seeming malignancy if not properly treated, to outline some of the clinical and pathological features, and above all to present a treatment, which in my hands has worked not only satisfactorily, but like magic.

This paper is based on the study of an interesting case which occurred at the Washington University Hospital about a year ago, the patient complaining of very frequent, painful urination, the passage of stones and bloody urine.

F. H. Mother died at menopause, one sister died during confinement. No bladder or kidney trouble in family; no history of tuberculosis or cancer.

P. H. In childhood had whooping-cough, smallpox and pneumonia; no scarlet fever, diphtheria or typhoid. No history of cardiorespiratory disease except pneumonia. She has always had a good appetite and digestion. She has been accustomed to eating large quantities of lemons, from seven to ten a day. Has never been a meat eater, but is a vegetarian; also eats large quantities of bread. Up to the present time she has had no trouble with her bladder. There has

been no history of gonorrhoea or syphilitic infections. Her menstruation has always been irregular, accompanied by considerable pain during the first day. Married eight years, has two children, aged seven and four respectively. Confinements, both normal, neither delivery instrumental, catheterized after the last delivery.

P. I. Began four years ago, one month after last confinement. The initial symptom was frequency of urination, with some straining at the end of the act. This frequency grew worse, until two years ago when she was forced to urinate every ten to fifteen minutes, day and night. Hematuria started three years ago, at first terminal, later entirely mixed with the urine. It has been constant, at times bright red, but generally chocolate color. About the same time she began to pass small stones in her urine, which has kept up intermittently since. There was considerable pain throughout; at first it was terminal, later continuous, so that the patient was never free from suffering. Between the urinary acts she felt as though something was pressing down on her bladder. Two years ago she consulted a surgeon, who did a suprapubic cystotomy, and according to the patient's statement he removed a tumor and stone from the bladder. The suprapubic wound healed in eight weeks. Patient was not relieved by the operation; her former condition recurred, characterized by frequency of urination, pain and hæmaturia. Tolerating this as long as she could, and in the meantime losing weight and strength, she presented herself at the Washington University Clinic, May 24th, 1913, in a very bad condition, suffering from the symptoms which have been described above. She was very pale and thin. On examination, her heart and lungs were found negative. Abdominal examination showed liver and spleen to be normal in size, but the right kidney palpable, though not enlarged or tender. There was a scar in the suprapubic region, site of previous operation, which was extremely tender. Vaginal examination was negative. Urine bloody, alkaline, Sp. Gr. 1018-1027.

Contained blood, pus, long bacilli, (Proteus vulgaris), hæmoglobin 80%. Wassermann reaction negative. Cystoscopic examination: There was considerable oedema of the meatus and whole urethra; slight click on introducing the cystoscope. Bladder capacity 150 cc. Bladder easily washed clean. Cystoscope showed a very terrific general acute cystitis. The bladder throughout was velvety and red. Over the trigone and bladder base were seven irregular, elevated projections, covered with a silvery white deposit. These masses were not composed of long villi, but the surfaces were irregular and covered with a downy material. The ureter catheter showed that the masses were fixed to the bladder wall, but that this downy material could be elevated and moved about. It was, however, quite superficial, the main bulk of the tumor being firm and fixed. Around the base of these tumor masses was an intense hyperæmia, and in several places bleeding spots could be seen. Around the internal orifice of the bladder, particularly below, there was an annular band of this same material, with the exception that it was not elevated. Neither ureteral orifice could be seen, as the whole trigone was obliterated by these masses. The urine was examined for tubercle bacilli, and guinea-pigs were injected, the results being negative. The diagnosis at this time was, "incrusted bladder tumors." Patient received copious amounts of water, urinary antiseptics, irrigations of bichloride and boric acid solutions twice daily. She was treated with high frequency sparks for some time without any benefit whatsoever. She continued to bleed and also to have the same distressing symptoms. A piece of tissue removed with an operating cystoscope was analyzed by Dr. Brooks, and his report is as follows: The specimen shows on its surface a large amount of necrotic, eosin-staining material. Imbedded in this necrotic material are small densely hæmatoxlin-staining bodies. None of these shows a definite crystalline form, but in some instances there is imperfect crystallization. The greater portion of this blue-

staining material is in the form of fine granules. Underneath this necrotic surface there is a zone of granulation tissue containing numerous new-formed blood vessels showing an extensive infiltration with leucocytes. The squamous epithelium of the bladder persists in small islands of cells. There are also occasional villous-like masses of epithelium made up of a central core of connective-tissue with a squamous cell epithelium covering.

Pathological diagnosis: Chronic ulcerative cystitis with multiple epithelial papillomata and deposit of calcium salts.

June 14th. Suprapubic cystotomy. The bladder was found extremely red and velvety. The whole trigone, internal orifice and bladder base were studded with small, irregular, tumor masses of different sizes, none larger than a small cherry, which were covered with a whitish granular material that seemed quite soft to the palpating finger. The internal orifice was surrounded by an annular mass of this material. There were no definite villous tumors, and no evidence of tuberculosis. The small tumors were softer than the larger ones. The larger ones yielded a gritty sensation on palpation; all were firmly fixed to the bladder wall and were removed by curetting and excision. The interior of the bladder was sparked with the high frequency current, and then closed in two layers; retention catheter for drainage. Patient did nicely after operation. Bleeding diminished for a short period, but she soon returned to her previous state. Cystoscopic examination two weeks after operation, showed the identical picture of the previous examinations, with the exception that there was a white deposit along the line of the suprapubic scar. Hæmaturia still persisted. Patient left the hospital for a while, but returned in November in identically the same condition, except that she was weaker and more anæmic. This time she had only about seventy per cent hæmoglobin. She was voiding every fifteen minutes, day and night, and suffering exquisite pain. She was curetted through the urethra several times, large masses of calcareous

material and granular debris being removed. Analysis by Dr. Shaffer showed the material to be composed of calcium phosphate, triple phosphates and ammonium urate. The urine was highly alkaline and the Proteus vulgaris bacilli were still present. She was put on boric acid irrigations, acid sodium phosphate and urotropin by mouth in large doses. By means of these curettages, internal medications, fulgurations, and repeated twice daily lavages, nothing was accomplished. The case really seemed hopeless, as after the removal of the material, this same condition would rapidly reappear.

Realizing then that I was dealing with a highly alkaline urine kept up by the Proteus infection with the rapid precipitation of calcareous material, efforts were then directed toward changing the chemical reaction of the urine, and maintaining this reaction. For this purpose, an emulsion of Bulgarian bacilli, prepared from the tablets of Hynson and Westcott were used. Three tablets were used at a time, which according to the laboratory report of Hynson and Westcott contain 18,000 bacteria. The first injection was given March 5th. This treatment was repeated daily for one week, then every other day for ten days. Much to my surprise, at the end of forty-eight hours the patient was greatly benefited. Hæmaturia had considerably diminished, she could retain her urine an hour, was passing large amounts of calcareous material, and was not suffering pain. On March 7th urine cloudy and still alkaline. Stained specimen showed pus and large bacilli in chains. March 11th, urine hazy. Cystoscopic examination at this time showed multiple ulcers on the bladder base, trigone and internal orifice. No incrustations or signs of tumor masses. Both ureteral orifices were easily seen and showed no ulceration directly over them.

Ureter catheters were passed easily within the orifices and perfectly clear acid urine without infection was obtained. March 12th, patient is able to hold urine three hours, but

urine still hazy. March 16th, patient slept five hours last night, urine very cloudy and very acid, and contains large bacilli. Bichloride irrigation. Forty-eight hours later urine clear, still acid. March 28th, a little over three weeks after first treatment, bladder is entirely free from any incrustation. There is no redness, except posterior to the trigone; there is a healing ulcer. Both ureteral orifices clean; trigone flat. The œdema of the urethra and the bladder base has entirely subsided. May 11th, urine still acid and clear, and has been so without treatment for 2 months. She can retain her urine seven hours, has no pain, and absolutely no urinary annoyance. She has gained a great deal in weight and feels like another individual.

This severe type of rebellious alkaline cystitis, accompanied by a rapid precipitation of salts on an ulcerated bladder and associated with such a protracted hæmaturia, is so very obstinate to the accepted treatments and was so simply controlled by the treatment as give above, that I felt that a report might interest this Association.

The pathogenesis of this disease is no more definitely understood than is that of incrustation elsewhere. It seems most probable that the combination of infection, necrosis and urinary super-saturation are the mose important factors, but as Lichtenstern and others have said, just why the organism should cause necrosis and incrustation in one case, and proliferative changes in another, is unknown. The bacteria which have been found in association with it are, proteus, staphylococcus, streptococcus, colon and the tubercule bacilli.

Incrusted cystitis has been observed as a concomitant disease with gonorrhœa, stricture of the urethra, chronic cystitis, hypertrophy of the prostate, bladder tumor, renal tuberculosis, pyelonephritis, around vesicovaginal fistulae and foreign bodies.

Its inception frequently occurs during the puerperium, the so-called puerperal cystitis, which was evidently the cause of the disease in the case described, as her initial symptoms

dated one month after the birth of a child. There was probably another etiological factor, as she had been a great lemon eater and vegetarian.

There are two types of incrusted cystitis: The flat or surface type; and the elevated or tumor-like. These may occur alone or associated. The first is more commonly seen behind stricture, prostatic obstruction, around the vesico-vaginal fistulae, and capping bladder tumors. When the two are associated, the flat or surface type is more commonly seen around the internal orifice of the bladder. The elevated or tumor-like is the rarer and more interesting of the two, and is due to the constant precipitation of salts on the already incrusted ulcer. In gross these masses resemble small, irregular tumors, studding usually the trigone and bladder base, covered with a soft, downy, whitish gray, granular material, which Marion describes as resembling a sponge. The small masses are soft; the larger ones quite firm. They are very adherent to the bladder wall, and upon their removal, a punched-out ulcer with irregular edges is left in the mucosa. The bladder shows a general, intense, acute cystitis, with marked edema. The slightest touch causes bleeding. This œdema is not confined to the bladder, but extends along the urethra and causes the same intense reaction at the meatus. Pyelonephritis is a common complication; this did not obtain in our case. The incrustation is usually limited by the submucosa, but in the more advanced cases the whole bladder-wall may be infiltrated.

The large masses resemble true stones; the smaller ones and the incrusted ulcers show a micro-pathology similiar to that described by other authors, who have studied this subject, namely: a superficial necrotic zone containing islands of incrusted material and thrombosed vessels, and beneath this a dense zone of infiltration. Here and there islands of mucous membrane remain, and in the case cited there were several villous-like projections which resembled vesical papillomata, but were evidently chronic proliferated changes.

These villous-like formations are frequently observed in chronic ulcerative cystitis.

The most distinguishing pathological characteristic of this disease, is its tendency to rapid recurrence. These tumor-like masses can be entirely removed, and within forty-eight hours completely replaced. The salty precipitate which is usually present in incrusted cystitis, is generally composed of phosphates. The incrustations occurring in an acid urine differ from this type in that they are more shell-like and when removed, do not show the tendency to tear.

The symptoms of this disease are usually those of a very aggravated cystitis, starting with marked increased frequency of urination, hæmaturia which is terminal at first but finally constant. In the case reported the hæmaturia persisted for three years without remission. The urine is generally highly alkaline and urination very painful, and a characteristic and almost pathognomonic symptom is the repeated passage of calcareous material in the urine.

The diagnosis is not always easy. The cystoscope offers our only intelligent means; and often cystoscopically one may be mislead as to the true nature of the condition. At first I was entirely wrong in the diagnosis as I thought I was dealing with multiple papillomata which had become incrusted, particularly in the light of the fact that the patient had undergone a previous suprapubic operation supposedly for tumor.

The tumor-like incrustations present an interesting picture, and are extremely difficult to differentiate from incrusted tumors. The chief points of difference are that they are not composed of long villi, are very firm, with surfaces usually irregular and downy, and that by means of the ureter catheter, only the superficial downy material can be moved about. Further, the removal with an operative cystoscope shows the masses not to be composed of villi but of inflammatory tissue and calcareous deposits. These elevated incrustations have been mistaken for true calculi. The diag-

nosis can usually be made by the fact that they are fixed to the bladder-wall, and occur in different parts of the greatly inflamed bladder. A point to be remembered is that the intense cystitis with such marked œdema makes it often difficult to predict the fundamental cause of the incrustation, for in our case, among other things an incrusted tubercular bladder was considered, but the usual standard tests for tuberculosis were negative. It is often impossible to locate the ureteral orifices, since these deposits so frequently cover them.

An important diagnostic point and one which cleared up the reported case, is the therapeutic test of acidulating the urine, thereby causing rapid evacuation of the tumor masses, demonstrating the remaining ulcers, quieting down the cystitis so that a more thorough inspection is possible, and permitting ureter catheterization in order to examine the upper tract.

It is concerning the treatment of incrusted cystitis that I wish particularly to speak. Naturally, with a disease so obstinate and so rebellious, one would expect to find many methods proposed. All the standard lavages and instillations have been utilized, namely, bichloride of mercury, lithium salicylate, silver nitrate, weak solutions of acetic and lactic acid, iodine, etc.; coupled with these, internal urinary antiseptics, particularly urotropin and its derivatives. None of these has produced lasting effects or curative results, and most of the afflicted individuals have been finally forced to undergo surgical interventions.

Surgically this problem has been attacked endovesically, suprapubically, and in rare instances vesicovaginally. The surgical principles which have been employed have been curettage, excision and drainage, the idea being to remove the calcareous material and to drain the bladder in order to prevent reformation. Curetting the bladder through the urethra has been employed by several men, notably Marion, Unterberg, Francois and others. This method has been used only in the female. By this means these authors claim that the bladder

mucous membrane can be thoroughly removed, and that cure frequently follows. After the curettage, a retained catheter is usually placed. This method is simple in execution and without particular danger; it possesses the extreme disadvantage, however, of being a blind surgical procedure, so that one knows neither the extent nor the thoroughness of his manipulations. It is attended frequently with rapid recurrence. Francois, in his report, shows fifty per cent, and in our case recurrence was almost immediate.

The suprapubic methods have been thorough curettage in an open bladder, curettage with the application of iodine and other chemicals, either with closure or with temporary fistulae; and finally excision of the tumor masses and ulcerations with suture of the mucous membrane, as has been done by Lichtenstern, Latzko, Zuckerkandl, and Marion. These suprapubic operations have been attended with a high per cent of recurrences, except those in which the ulcers were resected and the mucous membrane sutured. This procedure seems to heve offered satisfactory results. In our own case, I am inclined to think that recurrence would have followed, even had this procedure been utilized, as there was rapid recurrence along the line of suprapubic incision, following the bladder suture.

In fact all the above procedures were employed, and in spite of the utmost surgical care, our attempts were baffled, as recurrence was almost immediate. After this long line of surgical therapy, attended by such suffering on the part of the patient, a simple therapeutic measure, the intravesical injection of Bulgarian bacilli, sufficed to effect a rapid and complete cure. The Bulgarian bacillus has been employed in many parts of the body to combat infections. Since Metchnikoff first used these bacilli in intestinal putrefaction, other men have followed his lead and used it in various diseases, particularly in chronic nose, throat and sinus troubles, and chronic suppurative wounds. Gynæcologists have used it quite extensively in vulvovaginitis. North in 1909 collected

a large series of cases in which this organism had been used, and the results in many cases were quite satisfactory; but in his series of three hundred cases he makes mention of but two in which it had been used in the bladder. As far as I have been able to determine, this procedure has never been previously used in incrusted cystitis.

I realize that to the general profession this therapy has been considered with great levity and skepticism; I entertained the same idea until I began to use it. If one will stop to take into consideration an old adage which runs "Fight fire with fire," the rationale of this treatment will be elucidated. It is a known fact that certain species, and even strains of the same species of bacteria, are positively antagonistic to others as regards growth and development. An example of this is the well-known disappearance of the normal flora from the urethra of the male during the course of an acute gonorrhœal urethritis.

Growing in this patient's bladder were bacteria requiring an alkaline or faintly acid medium. Injection of any acid, mineral or organic, would have afforded only temporary change in reaction, because of the frequent evacuations of the bladder. In introducing acid-producing bacteria, we hoped that some would remain between the urinary acts and become implanted, multiply and create enough acid to transform the urinary reaction. In the bacterial battle which followed, the Bulgarian bacillus soon demonstrated its supremacy and shortly overwhelmed the Proteus, and within three weeks the desired change occurred.

Your time will be spared by my not entering into the morphological and cultural characteristics of this organism, as this has been thoroughly done by Metchnikoff, Heineman, Hefferan, Herter, Kendall and others. It will suffice in this connection to say that under suitable surroundings, particularly in the presence of carbohydrates, this organism is capable of generating as high as three per cent lactic acid.

Criticism may be offered that the technique of injecting an emulsion of these tablets into the bladder might be faulty for two reasons: first, the introduction of other bacteria, and second, that these bacilli themselves might be harmful. To defend the treatment against these remarks, it may be said that several cultures of these tablets were made by Dr. Thomas of the Bacterial Laboratory of Washington University, and on each occasion a pure culture of lactic acid bacilli was obtained. The first proof of the harmlessness of this organism to the bladder mucous membrane was demonstrated in the patient herself, who developed an acid urine containing these organisms, which after one bichloride irrigation were completely destroyed. They seem to lack resistance to chemicals, and even to their own acid products, because when a certain degree of acidity is reached they die. Several animals were used to demonstrate the effect of this organism upon the bladder mucous membrane. Two dogs were used. The bladders of these animals were injected, after suprapubic exposure with an emulsion of the Hynson and Westcott tablets and a forty-eight hour milk culture. One of the bladders was traumatized by crushing; the other was not, and in neither of these bladders was there the slightest inflammatory change. The urine at the end of four days was sterile. One rabbit was also treated in a similar manner with the same negative result. These experiments clearly indicate the harmlessness of this organism within the bladder.

I have used this treatment in several cases of alkaline cystitis; in one case of ammoniacal suprapubic wound, with very pleasing results. I should think it would be particularly indicated in the recurrent soft phosphatic calculi, occurring in alkaline cystitis which occasionally follows prostatectomy.

In conclusion I wish to say that in the treatment of incrusted cystitis, we are not "therapeutically helpless" as Unterberg, Lichtenstern and others have said, but possess in the Bulgarian bacillus a remarkable therapeutic power, which I believe worthy of your trial.

A CASE OF INCRUSTED CYSTITIS SHOWING END RESULT

By R. F. O'Neil, M. D., of Boston

THIS case is reported as of interest because we were able to observe the condition of the patient a number of years after an attack of this severe type of vesical inflammation.

The patient was a woman of 29, a native of Nova Scotia who entered the Massachusetts General Hospital April 28, 1896, for relief of hæmaturia, vesical tenesmus and urinary pain and frequency. She had been married nine years, had four children, the youngest being 14 months old. During her last pregnancy, she had an attack of typhoid fever; four months after this she was delivered of a living seven month's child. Her urinary trouble began about a month after delivery. Forceps were not used. Six months previous to her entrance to the hospital, she noticed she was passing bloody urine, at times with clots. She has passed blood more or less ever since with an occasional interval of a couple of days at a time. For about a month she had passed fragments of "gravel." There has been for six months constant pain in the lower abdomen and urethra which is much worse at the time of micturition. The interval between micturitions is never greater than one-half hour.

Physical examination: Fairly well developed woman. Heart, lungs and abdomen negative except for extreme tenderness over bladder. Patient too sensitive to permit vaginal examination. Pulse, temperature and respirations normal. Urine red, cloudy, alkaline, specific gravity 1020, albumen large amount, sugar absent. Sediment. Principally blood and a few epithelial cells.

April 30, 1896. Bladder examination under ether. Urethra dilated and bladder examined with finger. Calcareous patches felt in bladder wall, especially in region of trigone. Bladder speculum introduced and viscus inspected by reflected light. Large grey sloughing patch on trigone and other smaller areas scattered over wall, especially at base. Summit practically clean. Much bleeding from bladder wall. Catheter passed into ureters with some difficulty because of slough in region of trigone. Bladder mucous membrane curetted and cavity irrigated. Some reaction after operation. T. 101. P. 106, and a good deal of pain.

The condition gradually improved under irrigations and rest and she was discharged May 30, one month after operation, much improved as to general health and local condition. There was slight pain only on micturition and the interval had increased to three or four hours.

On the evening of Jan. 27, 1914, the patient again entered the hospital complaining of abdominal pain, principally on the right side, history of chills, fever and delirium for several days. Some frequency of micturition, day one-half hours, night one or two times. Temperature on entrance 102.8, pulse 108. White count 11,000. Chest negative. Abdomen flabby, tympanitic, general right sided tenderness. No muscular spasm. A small mass felt in region of gall bladder. Vaginal examination negative. Urine, high, cloudy, acid. Albumin slight trace, hyalin and granular casts, a few leucocytes.

After a few days in the hospital the temperature fell to normal and the abdominal tenderness disappeared. X-ray examination negative. At times the urine showed a little blood and a slight trace of albumen.

In view of the old history of cytitis, the Genito-Urinary Department was asked to see her in consultation as to the kidney having been the cause of symptoms.

Cystoscopic examination, Feb. 10, 1914. Bladder shows evidence of inflammatory process in the past, mucosa thick-

ened in places, an abnormal fold at the base, mucous membrane on the whole paler than normal. Ureteric orifices normally placed and normal in appearance. Catheters passed easily to each kidney, normal looking urine appeared from each side. Sediment shows no pus from either kidney and no growth. Renal function, phthalin 1 c. c. intravenously. Time of appearance on right 4½ minutes, on left 5½ minutes. Amount recovered in 15 minutes, right 8 per cent, left 14 per cent. There is no evidence of an inflammatory condition of either kidney. There was some frequency the night following the cystoscopy but she was discharged two days later, Feb. 12, 1914, micturition being normal. The urinary findings and symptoms on entrance are easily accounted for by the pyrexia.

In a conversation with the patient May 13, 1914, she stated that following her discharge from the hospital in 1896 she suffered from recurrences of her bladder symptoms with passage of blood and calcareous masses, sometimes of good size, for a period of four years. At times, the attacks being nearly as bad as when she entered the hospital. The condition gradually improved and disappeared under local treatment and she has had little or no trouble for the last ten years. At present there is no pain and no nocturia. She describes herself as having "a little bladder weakness, but in other respects her bladder being better than ever."

From the clinical history and operative findings, this case was evidently one of so-called incrusted or calcareous cystitis. The point of interest being that so severe a process could persist for such a length of time, between 4½ and 5 years, terminate in recovery and leave little or no permanent disability of the bladder, also that during the time, infection of the kidneys did not occur at least to any permanent degree either by way of the ureter or the lymphatics.

DISCUSSION

Dr. Louis E. Schmidt, of Chicago: I have had occasion to observe several cases very closely in the past year, and I would like to report one in detail. A woman about fifty-five years old had been a sufferer from urinary symptoms for a great many years. She had had frequency and tenesmus, and in addition to that later on she was required to take up catheterization as she had considerable retention. I saw her some nine months ago for the first time, and on examination both kidneys were palpable, but particularly the right one, which was increased in size. The cystoscopic examination showed an unusual condition. A large part of the bladder, practically all of the bladder except the upper part, was covered with a condition that resembled a frosted window. I do not know anything else that it could be compared with. It was a peculiar crystaline formation. At the edges, which were very regular and could be easily outlined, was a distinct inflammatory zone. The urine contained very little pus. There were crystaline bodies that could be washed out, and on chemical examination it was shown that they were carbonate of lime. The bacteriological examination showed the colon bacillus. No other organism was present. The retention of urine in this case was about eight or ten ounces. I put her on regular catheterization twice a day, and used dilute hydrochloric acid for irrigating purposes. Internally I gave her dilute hydrochloric acid three times a day, and made an auto-vaccine of the colon bacillus, which she is still receiving. I examined her about nine months ago, kept her under observation for about six weeks in the hospital and during that time I curetted the bladder with the aid of the cystoscope. I used an ordinary dull uterine curet, which was introduced along side of the cystoscope, and I could roll up this peculiar membranous substance. After washing out the bladder with dilute hydrochloric acid and after the patient had taken the remedy internally the crystaline condition disappeared and there was only the membranous condition remaining. I did this two or three times a week for about four weeks, she went home for four months. The patient has just been in the hospital again and I have repeated the procedure. She gets over the crystaline condition, but this membranous film, which extends over the greater part of the bladder, is still present. The edges roll up and certain portions are removed, and then it reappears, but without this peculiar incrustation. In all

these years she has had typical attacks of renal colic, with pain and high temperature. However, since she has been on this treatment she has been greatly improved in every respect. There is practically complete absence of pain in both renal regions, and apparently the right kidney is decreased in size. Her urinary symptoms are greatly improved. Both ureters were catheterized, and the colon bacillus was found on both sides, nothing but a certain amount of pus and a few red blood corpuscles being present. The urine at the present time as far as the macroscopic appearance is concerned is almost clear. Bacteriological examinations have been repeatedly made, and the same organism found, no other organism being present. At both ureteral orifices there is a peculiar teat-like arrangement that you might say looks like a neoplasm, but it is sessile and is also covered with this incrustation, and seems to act rather as a ureteral caruncle. On touching this, extreme pain is elicited. The rest of the bladder is not unusually sensitive.

I am under the impression that in all of these cases—and I could quote many of them—the condition also exists in the higher urinary tract, as in this particular case. I had occasion two or three years ago to watch one of these cases, and in one of the early cases I also made a suprapubic cystotomy and also noted the same results. There was complete recurrence in a very short time. For this reason nine months ago I followed out the treatment outlined in the foregoing case.

When you roll off this peculiar membrane there is a peculiar velvety appearance and absence of the mucous membrane, and this inflammatory condition probably extends deeply into the muscular tissue.

If one is fortunate enough to be able to have such an infection as Dr. Caulk mentions and get such a result it is wonderful, but I do not believe simply because he has gotten this result in one case that it is possible to get it in all cases, because the case I have mentioned simply shows the fallacy of that belief. It is well worth while trying, but I have about the same idea as Dr. Caulk, that it is undoubtedly due to an internal disturbance of some kind, in the first place; secondly, a urinary infection must be present. If one can rid the individual of this particular kind of infection there is a possibility of improvement.

Now, the bacteria are still present in the case referred to and if it were possible to keep up the treatment sufficiently long the result might be different; but from the observation that I have given to this individual case I do not expect much of a change. If I hold the patient in this comfortable state I think I will be perfectly satisfied. It is decidedly

better, and as I say, on cystoscopic examination there is only a film, and this peculiar incrustation has disappeared.

In connection with incrusted cystitis I think it is well worth while making a particular classification. I do not believe I would consider those cases of cystitis with incrustations belonging to this group. They are simply nothing more than phosphatic deposits, as a rule, and real incrustations are where there is present a distinct crystaline formation based on conditions aforementioned.

Dr. Arthur L. Chute, of Boston: I would like to ask Dr. Caulk whether there was a residual constantly in his patient. I saw a woman last winter who interested me, but who had nothing like the amount of incrustation Dr. Caulk's cases presented. The history of my patient, a widow of 62, went back to the winter of 1912, when I saw her for the first time, with a tremendously irritated bladder. I examined her cystoscopically and told her doctor there was little to do, that she had an advanced carcinoma and could not live long. A year afterwards he telephoned me that under the treatment I had suggested this patient had improved, but could not pass urine voluntarily. He asked me if there was any possibility that I could have been wrong in my diagnosis. I admitted that it seemed more than possible. I sent this woman into a hospital, where, besides her complete retention, I found that there was a little patch just behind the trigone that had a little incrustation upon it. I could find no satisfactory reason for her retention. She had a blood pressure of 230 mm. hg. and increased reflexes, but no clonus. Dr. Edw. W. Taylor went over this patient for me and gave his opinion that she had a slight lesion in her pyramidal tract that stimulated both the detrusor and the sphincter, but the latter to the greater degree. She still has the residual and still has to be catheterized. My impression was that the incrustation was due to an alkaline infection of her residual with a deposit of lime salts on a little ulcerated spot, and that if we could get rid of the residual the incrustation would take care of itself.

Dr. Francis R. Hagner, of Washington: I think this is a very valuable contribution. It is along the same line that Young at Hopkins some years ago was working, that is, in the implantation of an acid producing organism to overcome infections from bacteria producing an alkaline reaction.

I had a case of proteus infection of the bladder in a negro woman. Nothing seemed to improve her condition. I tried to infect her bladder with the colon bacillus. We gave her repeated injections of the colon

bacillus into the bladder, and even made traumatism, but we never could get them to grow. I simply cite this as being along the same line of treatment.

Dr. Edward L. Keyes, Jr., of New York: I happen to have seen three cases of incrustation of the bladder in the past two years, and I mention them, though they are not a particularly glorious record, simply to add to the cases reported today. One was a man who was operated upon in my service by one of my assistants for a gonorrhoeal prostatic abscess. A week after operation he had a sudden and most violent hemorrhage, for which he had to be anesthetized and repacked. After a prolonged septic convalescence he finally healed and went home, but still with a great deal of inflammation of the bladder (without residual urine) not relieved by local treatment. Several months later I cystoscoped him, and found very small incrustations around the bladder neck. I tried various washes in vain. I then did a suprapubic section and scraped off these incrustations. Once again he bled in a most glorious manner. I packed his bladder, but he succeeded in dying of his sepsis two weeks after the operation.

In a second case, a woman, a multipara, the inflammation of the bladder seemed to date from her last pregnancy. She had had the hemorrhages, which were a part of the clinical picture, for two or three years when she first came to me. There was a very violent cystitis. The cystoscope showed an incrusted ulceration on the trigone which looked quite like a carcinoma. I helped her considerably by treatment with the forceps of an operating cystoscope, but never succeeded, in the course of two or three months, in getting rid of the incrusted ulcers. Then I put her in the hospital and curetted the ulcers. I cystoscoped her immediately thereafter and found a few small incrustations remaining, picked them off in one sitting, and she has remained well for the eight months since that time. She also had no residual urine.

In a third case I did a suprapubic prostatectomy. The patient, though emptying his bladder, continued to have a good deal of irritation for an undue length of time after operation. I cystoscoped him and saw two small ulcerations near the bladder neck, both of them incrusted. I picked the phosphates off with the forceps through the cystoscope and succeeded in curing him after about six or eight treatments.

I submit therefore, in the first place, my admiration for Dr. Caulk's contribution, for that will take the place of all these methods in case it makes good. If, however, some of these infections cannot be over-

come by symbiosis, I submit that curetting, through a suprapubic section or through the urethra in the female, ought to succeed if it is followed up by cystoscopy and intravesical operation to get rid of the little relapses which are likely to occur.

DR. CAULK: The remarkable thing, as Dr. O'Neil mentioned, was the restoration of the bladder function. At the end of three weeks, after treating this terrific looking bladder by an emulsion of bulgarian bacilli, the bladder cleared up and looked remarkably well. As a matter of fact, on first inspection one would hardly appreciate the fact that anything had happened to it, except for the one healing ulcer, and the patient's bladder was functioning almost perfectly. The patient did not have any retention, although that is a very frequent symptom in the disease, and she had no residual urine. One striking thing about this case was the edema of the meatus, demonstrating such an intense infection. The whole urethra and meatus were blown up with an intense œdema.

I notice that Dr. Chute and Dr. Keyes both mentioned that they had thought their cases were carcinoma. That is exactly what I thought at first. I had never seen the picture before. Every time we looked at it we made a different diagnosis. It was really a very beautiful picture. We tried every method that we could think of to relieve the condition, but every effort proved futile.

Speaking of following up the curettages, I curetted her for a while every day after dilating the urethra. The poor girl suffered torment.

The kidneys in this case were not infected, at least at the time we did the ureter catheterization. It is safe to say that if they had been infected previously they probably would have been at the time, because we had done nothing at all to the renal pelves. I injected an emulsion of these tablets in a double alkaline pyelitis. It cleared up the infection temporarily. That was just recently, and I have not seen the man since.

Recently I had a case of carcinoma of the bladder, a very malignant carcinoma, which was too late for any chance of cure, in which I did a suprapubic drainage. He got a dirty sloughy foul smelling suprapubic wound. I tried to clean it up, but without any benefit at all. I took pure lactic acid and painted around the wound, and it cleared it up beautifully, then I packed the wound with gauze saturated with an emulsion of these tablets. The patient has been much more comfortable,

the room has not smelled anything like it formerly did, and altogether the treatment was beneficial.

The treatment of incrusted alkaline cystitis by means of the bulgarian bacilli is almost magical. I could hardly believe the result and was almost as startled as you all probably are, but in the soft recurring phosphatic precipitate of this type, it is really remarkable to see the rapid result.

Dr. O'Neil: The case that I reported was operated on in 1896; at that time we were not doing much with the bacteriology of the urine, so we cannot tell anything about the infection, except that she had an alkaline urine. Whether her previous typhoid had had any connection with it, I do not know. She did not know what local treatment she had received but whether because of or in spite of any local treatment she had gotten entirely well at the end of four years.

URETEROCELE AND URETERAL STONE

By B. S. Barringer, M. D., of New York City

THE more commonly recognized kidney complications due to ureterocele are those of obstruction, hydronephrosis or pyonephrosis. Kidney or ureteral stone are rarely if even spoken of as being caused by ureterocele. If they occur at the same time, as in two of the six cases reported by Caulk, (Uretrovesical Cysts, Journ. *A. M. A.*, Nov. 8, 1913) the passage or presence of the stones is considered the cause of the ureterocele. This would seem to be confusing cause and effect. It is difficult to see why a stone passing through a ureter should cause a stricture at this one terminal point, and carefully neglect every other part. On the other hand the urinary stasis caused by the uretrocele certainly ought to favor the formation of stone.

In this paper I wish to draw attention to the possibility of kidney stone being secondary to ureterocele, perhaps oftener than is commonly thought. When there is this dual condition our attention should be first centered upon excision of the ureterocele before the kidney or ureteral stone is attacked. This presupposing that the kidney is good enough to be saved. A similar procedure is the elimination of a bladder retention before any kidney operation is performed. Caulk's method of resection of the cyst has been slightly modified. A portion of the cyst has been removed with the operative cystoscope, enough tissue being excised to thoroughly open the ureteral orifice. This would seem better than simply slitting the cyst, as some surgeons believe that this latter method gives but temporary relief and that the cyst reforms.

PLATE IV.—Presented by Dr. B. S. Barringer

Fig. 1—Stone caught in ureterocele

Fig.2.—Cutting away margin of ureterocele with cystoscopic forceps.

Fig 3.—Appearance of ureteral orifice five days after operation shown in Fig 2.

The following case illustrates: Mrs. S. had, four months ago, a severe attack of pain in her left groin lasting one half hour. Since then she has had two similar attacks. Her urine was reported as containing albumin and a radiograph was said to show a stone near her left ureteral orifice. Cystoscopy showed the left ureter bulging and reddened, and through the small orifice, a stone could just be seen. She had an incipient ureterocele of her right ureter. I introduced an operative cystoscope, and tried to grasp the stone. It slipped out of reach into what I believed was the pocket of a ureterocele. I say believed advisedly as under these conditions diagnosing an incipient ureterocele is difficult. The fact that the patient had a ureterocele of the opposite side was added evidence, as ureteroceles are often bilateral. With the forceps of the operative cystoscope I took two bites from the margin of the ureter, so making an orifice which I subsequently found was 11 or 12 French. The stone was still in the ureter so a ureter catheter was passed beyond it and by twisting the catheter on its long axis the stone was delivered. The patient passed the stone into her urethra where it stuck, but was easily taken out with an artery clamp. Two months later the closed beak cystoscope of the forceps (11 French in size) was introduced into the normal left ureteral orifice so demonstrating the efficiency of the operation. At this time two bites were taken from the margin of the right ureterocele.

The pain of this operation is moderate. At the time the bite is being taken sharp pain occurs. No anæsthetic fluid has been introduced into the bladder because the effect of such fluid is superficial and would probably help little. These incipient ureteroceles are more frequent I believe than even the most recent literature would indicate. There is naturally no question of missing an outspoken well developed ureterocele if a cystoscopy is made. The irregular forms, however, are more elusive and the diagnosis is made not from the size of the ureteral orifice, which may be a pin point two, three or four French, but by careful observation of the bulging of the

intravesical part of the ureter occurring with every efflux of urine.

Intramuscular indigo-carmine might give enough color to the urine to make this bulging more noticeable.

A second case in which I have used the operative cystoscope as outlined above to remove part of the ureteral orifice demonstrated how efficient this method is in hurrying the passage of the ureteral stone, and also how rapidly the orifice heals.

Mrs. X. gave a history of pain in her right abdomen which suggested kidney or ureteral origin. A good radiograph was negative for stone. She was cystoscoped; the right orifice was bulging, much inflamed and in the center was a yellow mass looking like stone. The smallest olivary catheter could not be passed beyond this. The mass was grasped with the forceps of the operative cystoscope and a part of it pulled off which proved to be a stone. Two or three bites were taken out of the ureteral margin but even now the stone was too soft and too firmly implanted to be removed. Four days later she was reported to have passed a clot of blood containing something looking like a stone. A day later she was re-cystoscoped. The right ureter was covered with absolutely normal mucous membrane. There was no trace of a ragged edge; it was simply a little longer than usual. A catheter was passed through the ureter to the kidney.

It is possible in cases of recurrent uretral stone that a cutting away of a part of the uretral orifice may have some effect in preventing the formation of other stones. This even if a ureterocele is not present.

DISCUSSION

Dr. Arthur L. Chute, of Boston: I should like to ask Dr. Barringer if he thinks all the cases where we see the ureters working hard,—sort of laboring—have small ureteroceles? I gathered that he did.

Dr. John R. Caulk, of St. Louis: I was very glad to hear Dr. Barringer report such favorable results from endo-vesical manipulations. During the past year I have seen two other well defined ureteroceles. One I did not get a chance to operate on. The other I did suprapubically, because I wanted to be fair and make a comparative study, and I must say that the results are good in both. I could not see any difference. They both, as far as the late results are concerned, were excellent. Neither one has shown any recontraction. One of the cases that I operated upon endovesically, and the one that I did suprapubically, had associated with them very large dilated kidneys, one infected, the other not. I feel that there is a point to be learned from these cases, and I was glad to hear Dr. Barringer mention it, that is, to treat the ureterocele and not the kidney, as some men have proposed. These kidneys were both in bad shape. After the obstruction was removed I found in the infected kidney that the function was very poor, but about a year after the endovesical operation the kidney was removed, because, as I said in my report last year, the patient had a large ventral hernia and she was beginning to have pain in her side. The kidney, however, was a pretty normal looking one, with a pelvis but slightly dilated.

A recent case of ureterocele on which I did a suprapubic also had associated with it a vesical diverticulum and a urachal cyst, showing its congenital origin. This case had a good sized hydronephrosis and the patient was suffering great pain in the right renal region. After the cyst was resected suprapubically, even though it was doing nicely and draining well, the patient still had her pain. Last Tuesday I explored the kidney and found a perfectly normal looking kidney, with no dilatation of the ureter. I did a nephrotomy with the hope of relieving the pain, and found the cortex perfectly normal, and practically no dilatation of the renal pelvis. In other words, even after kidneys have suffered long continued dilatation for obstruction, the removal of the obstruction may allow them to assume fair function and be spared nephrectomy.

Dr. Louis E. Schmidt, of Chicago: In regard to this intravesical operative procedure. I operated a man fourteen years ago intravesically

and removed a stone with the operative cystoscope, and he happened to come into the office two months ago in regard to his wife. I took the opportunity to look into his bladder to see how that ureteral orifice looked. There was absolutely no contracture. The ureteral orifice was large enough to stick in your small finger. He is evidently perfectly well, there is no infection, and I do not think the condition is any detriment to him. Some people have objected to cutting the ureteral orifice on account of the fear of contracture. But in this particular case there has been no ill result whatsoever.

Dr. Hugh Cabot, of Boston: I am in accord with Dr. Barringer in thinking this a very important question. I believe that these congenital strictures of the ureteral orifice are more common than we have supposed.

My first experience, some years ago, was with a very much dilated one which ballooned up one whole side of the bladder. I entirely mistook the condition and did a suprapubic cystotomy, found what the condition was, resected the orifice, and relieved the pressure on that kidney. The kidney had been practically destroyed, and was afterwards removed and contained a stone.

Certainly in some of these cases the stone formation is clearly secondary to the obstruction, and I have been inclined to regard this malformation as an important factor in some of the stone formation which we see. The amount of obstruction often is small, but it is sufficient, I think, to give the starting point of stone formation, possibly by the medium of slight infection, slight ulceration, slight incrustation, and then the tide turns to stone formation.

Clearly if we can deal with these conditions equally efficiently by the intravesical method, it becomes at once the method of election. My own experience has been with the suprapubic operation, because my cases—five, I think, in number—have all been extreme. I have seen one or two of what might be called early cases, in which the dilatation was not great, and it was my own lack of skill with intravesical operations which prevented me from adopting Dr. Barringer's methods, because I clearly believe them to be the method of election in early cases.

I think we shall all be interested in watching in the future the effect of these operations upon the ureteral orifice upon kidney infection. I have recently been considerably interested in injury of the ureter as a source of kidney infections. Some very nice experimental work of Sutton and Stewart of Philadelphia points a line of investigation

which is very much in accord with my own experience, that kidney infections, particularly those of the renal pelvis, are frequently ascending by the route of the lymphatics in the ureteral wall. Their experimental work shows clearly that this is a source of infection. It has been brought sharply to my mind by the very inevitable infection of kidneys where uretera was resected and reimplanted, and infection of the bladder and infection of the other kidney did not occur. I believe that is probably a more common source of kidney infections than we have believed; that the ureteral lymphatics very readily take up organisms and that they travel directly through the lymphatic system to the renal pelvis, starting a pyelitis, and from that on, of course, the rest of the picture, pyelitis and perhaps metastatic interstitial foci in the kidney. It, of course, bears only indirectly upon the problem. The stricture must be treated, but I think there may be danger of subsequent trouble in the form of kidney infections, and I should like to see this question followed by those gentlemen who are skilful in intravesical manipulations.

Dr. George S. Whiteside, of Portland, Ore.: I should like to ask Dr. Barringer if the contraction of the ureteral opening takes place first and the formation of the stone occurs later; does the stone form where you find it, at the lower end of the ureter, or does the stone form in the kidney and afterwards pass down to the place where you find it?

Dr. R. F. O'Neil, of Boston: I should like to mention a case which occurred in our clinic about two years ago, that of a man of about 40 who, during a routine cystoscopic examination for renal stone, was found to have moderate sized bilateral ureteroceles. It was not apparent that these dilatations were causing any back pressure, but they made catheterization of ureters very difficult. He had a nephrotomy performed for stone. The kidney was more or less hydronephrotic and several calculi were removed. He turned up about a year later with the cystic condition in the bladder the same, but with another crop of stones in the same kidney. That the ureterocele in this case had anything to do with the recurrence of the stone does not seem to me probable unless by helping to cause the somewhat hydronephrotic condition of the kidney.

Dr. Barringer: In answer to Dr. Whiteside's question, I will say, "no one knows." I think that in the cases I have seen the pain of the renal colic beginning rather high and slowly coming down to the groin

would probably indicate that the stones formed in the kidney pelvis; but I think it is a question impossible to answer.

In answer to Dr. Chute's question as to whether all patients have ureteroceles, I do not think so. I think, however, that these incipient forms are fairly frequent and are fairly hard to diagnose. They simply show a little bulging external to the ureter. I think that indigocarmin injected subcutaneously might color the urine and so make the bulging more evident.

A CASE OF DIVERTICULUM OF THE BLADDER

By F. R. Hagner, M. D., of Washington, D. C.

THE following case of prostatic obstruction with complete retention presented himself October 8, 1913. Age 74, father had bladder disease and an uncle had died from prostatic obstruction, complicated by a large diverticulum of the bladder.

The patient had enjoyed perfect health up to 10 weeks prior to my first seeing him, when suddenly complete retention of urine occurred—this continued for over 48 hours with no discomfort to patient.

After an unsuccessful effort to catheterize him he was sent to a hospital where according to the statement of his physician, 144 oz. of urine were withdrawn. Within the next 24 hours, patient became unconscious and for some hours had complete suppression of urine. Under treatment the kidneys began to secrete and he was able to leave the hospital in 10 days. From the time he left the hospital until he saw me he was catheterized 3 times daily, never voiding any urine.

Rectal examination showed a fairly large prostate—abdominal palpation revealed a large bladder with a sausage shaped tumor extending upwards toward the right lumbar region. On catheterization 65 oz. of urine was obtained,—cystoscopic examination showed an irregular intravesical prostate with lateral and median enlargement and a definite post-prostatic pouch. A small diverticulum was seen on the left side of the bladder with the opening just to the outer side of the ureteral opening, on the right side of the bladder, a large diverticulum was observed with the opening at a point where the posterior and lateral wall of the bladder meet.

Prostatectomy was advised and performed (suprapubically), patient making a good recovery. I had advised removal of the large diverticulum, as I felt otherwise he would still have residual urine. As an experiment just before the suprapubic wound healed I filled his bladder with fluid through the urethra, closing the suprapubic wound with my finger; he was then able to pass with force a considerable amount of the injected fluid. He was then catheterized and showed 16 oz. residual .

Realizing the diverticula were responsible for this residual, I again filled his bladder and had him stand up and pass out all fluid possible (the finger keeping suprapubic wound closed), then he was cystoscoped through the suprapubic wound. This was easily done as there was 16 oz. in his bladder. Patient was then directed to make an effort to urinate; immediately—the light was obscured by the meeting of the bladder wall, no fluid escaping from the urethra or the suprapubic wound. He was told to continue his effort to urinate, and the cystoscope was directed into the large diverticulum which could be easily cystoscoped while he strained. As soon as he discontinued the effort to urinate the fluid would run out of diverticulum into the bladder when the diverticulum could no longer be cystoscoped on account of its collapsing. The smaller diverticulum holds very little, the larger one holds approximately 16 oz.

This patient is apparently in excellent condition; can hold his urine during sleeping hours, has no frequency during the day and passes a good forceful stream.

He always has 15 or 16 oz. of residual urine and refuses any further operative intervention at this time as he is perfectly satisfied to be able to urinate naturally.

I made an effort at the time of prostatectomy to evert the large diverticulum just as you would turn a glove inside out, but adhesions prevented.

I was able to do this in one case, removing the diverticulum by excising it from within the bladder.

OBSERVATIONS UPON TEN CASES OF DIVERTICULUM OF THE BLADDER

By Hugh Cabot, M. D., of Boston

IN 1912 I presented before this Association some observations upon three cases of diverticulum of the bladder and I desire to add to the evidence at that time put forward. The two most interesting questions in connection with these pouches are: (1) Their origin, whether congenital or acquired, and (2) Their management by operation.

Clinical Evidence Bearing Upon the Origin of Diverticula

In the discussion in 1912 I found myself at variance with the views expressed by Dr. Chute, to the effect that these conditions were acquired rather than of congenital origin. The acquired type must obviously be due to obstruction in the urinary tract at some point below the bladder and it behooves the advocates of the theory that they are acquired to produce evidence of obstruction as it also behooves those who hold the view that they are congenital to show that these patients are not guilty of obstruction. Upon this question I have the following evidence to submit:

Case I. J. H. G. 38 years. March, 1911. Symptoms began at age of 25. Autopsy showed bilateral diverticula with dilatation of both ureters, obstructive thinning of kidney tissue. Pyelonephritis. There was no demonstrable obstruction in the urinary tract.

Case II. M. G. 30 years. May, 1912. Symptoms six months duration. One diverticulum just above left ureteral

orifice. Removed by operation. No urethral or prostatic obstruction. Operative recovery. Patient lost sight of.

Case III. L. L. 28 years. October, 1909. Known to have had cloudy urine without obvious cause for two years. One diverticulum just above right ureteral orifice. Removed by operation. No urethral or prostatic obstruction. Patient well. Clear urine 1913.

Case IV. J. W. 29 years. June, 1913. Symptoms a year and a half. One diverticulum posterior wall of the bladder. Removed by operation. No urethral or prostatic obstruction. Good operative recovery. Patient lost sight of.

Casse V. A. G. 59 years. August, 1913. Symptoms one year. Prostate moderately enlarged by adenomatous growth. One diverticulum just above orifice of right ureter. Operative removal of diverticulum and prostate. Slow but satisfactory convalescence. Urethral obstruction due to prostate.

Case VI. W. R. B. 46 years. March, 1914. Cloudy urine of some years duration. No other symptoms. Operative removal of left-sided diverticulum, into which the ureter opened. No urethral or prostatic obstruction. Patient still under observation.

Case VII. G. B. C. 23 years. January, 1913. Symptoms two years duration. Cystoscopy shows bilateral diverticula in neighborhood of ureteral orifices. Kidneys too far gone to permit operation. No urethral or prostatic obstruction.

Case VII. G. B. C. 23 years. January, 1912. Sympeight years. Two diverticula on right side, one above, one below ureteral orifice. Operative removal. No urethral or prostatic obstruction. Operative recovery.

Case IX. E. F. P. 50 years. March, 1914. Symptoms ten years. Seven years ago perineal prostatectomy. No relief of symptoms. Ten ounce residual continued. Two right-sided diverticula above ureteral orifice. Operative removal. Operative recovery. No urethral or prostatic obstruction. Empties bladder completely.

Case X. A. N. B. 72 years. July, 1913. Symptoms ten years. Prostate much enlarged. Right-sided diverticulum some distance above ureteral orifice. Operative removal of prostate and diverticulum. Now empties bladder completely. Urine clear. Urethral obstruction due to prostate.

Taking these ten cases together they show an average age for the onset of symptoms of 37 years and though it is probably fair to assume that the condition must have existed for some time before the onset of symptoms, let us assume that it did not. Only two of them showed clinical evidence of obstruction, Cases V and X. Moreover, obstructive lesions of the prostate are, to say the least, not common at the age of 37 and none of these patients had any demonstrable stricture of the urethra. For the other eight cases it seems to me logically to follow that the advocates of the theory of acquisition must assume that the cause lay in fetal life and has disappeared beyond the possibility of clinical demonstration. This is, I believe, the theory of Englisch. It must, however, be admitted that a condition giving rise to sufficient obstruction to force outward all the coats of the urinary bladder and then disappear, leaving no trace behind it, belongs in the category of the miraculous. Upon the evidence submitted, and which I have been able to collect from other sources, it seems to me far more probable that these sacculations are of congenital origin, that they may, and do, exist for years without causing symptoms which, when they appear, depend upon the advent of infection. Entire quiescence must not, however, be taken to mean that these conditions are harmless since, as is well recognized, they occur most commonly in the neighborhood of the ureteral orifices. By their presence and by their extreme likelihood of producing pressure upon the ureter, they sooner or later produce ureteral dilation, hydronephrosis and extensive destruction of the kidney. When thus situated they are therefore always a serious menace to renal integrity and not uncommonly

destroy the patient. Those cases which have been discovered early and removed have been followed by complete cure.

Operative Management

It goes, I believe, without saying, that when a diverticulum of the bladder is discovered the indications for its operative removal are clear if the condition of the patient is such as to make recovery within the bounds of probability. Two conditions in particular, apart from mere technical difficulties, will influence the result. First, the condition of the kidneys and second, the amount of pericystitis and peridiverticulitis, which result from long-standing infection. In Cases I and VII the extent of the kidney damage was such as to constitute a positive contra-indication. Case I came to autopsy and Case VII was clearly not likely to live long. A lesser degree of the same condition existed in all of the other cases except Case III and Case X and must therefore generally be reckoned with. The damage to the kidneys resulting from obstruction is not confined, in the case of unilateral diverticulum, to one side. With diverticulum there has in our cases been constantly observed very marked hypertrophy of the bladder wall, which is sufficient to increase the pressure around the ureteral orifice on the sound side and thereby increase the work and lead to dilatation of the ureter. This bilateral dilatation was demonstrated at operation in Cases IV and VIII, both of which were unilateral cases. Upon precisely what mechanism this hypertrophy depends is not to me clear but its extensiveness I believe to be beyond question and its importance as a menace to the integrity of the kidney is certainly great. It therefore becomes extremely important to study the kidney function of these patients carefully before undertaking operation, though successful results may be obtained in patients with extremely low functions, as shown by Case VIII, whose function was at no time above 10 per cent by phthalein and whose average function was about 5 per

cent. Case V had a function of not over 15 per cent and Case IV, with a posterior wall diverticulum, a function of 25 per cent.

Pericystitis and Peridiverticulitis

The inflammatory reaction around these sacs is in some cases very great and has seemed to depend directly upon the duration and severity of the cystitis and diverticulitis. It not only greatly increases the difficulties of operative removal, which in all conscience are great enough in many cases, but when of severe grade and widespread, seriously compromises the future ability of the bladder to take up its function. This is aptly illustrated by Case VIII, in whom the removal of the diverticula, which were extensive though unilateral, did not result in return of the contractility of the bladder. He has, during the year which has elapsed since operation, been free from his acute attacks of pyelonephritis but his bladder still contains much residual and he is largely dependent upon a catheter. It therefore follows that the earlier these pouches are discovered and removed the greater the probability of a complete cure, such as resulted in Case III.

Operative Technique

I believe it to be now fully demonstrated that no operation short of excision of the diverticulum can be expected to cure. Case IX had his prostate removed years ago with great completeness but without benefit to his symptoms as he continued to have a large and increasing residual. Once these pouches have lost their contractility and give rise to residual urine that contractility cannot be restored by removal of an obstruction should such exist. The lesser operations of enlargement of the orifice of the diverticulum seem to me total failures. In one of our cases, Case VIII, this was tried three years before my operation, without benefit and without even stopping the progress of the symptoms.

I shall therefore confine this discussion to the technique

of excision of the diverticulum. In all cases, no matter where situated, the operation will be facilitated by the opening of the bladder, which not only enables the surgeon to identify accurately the position of the sacs but facilitates the dissection by what might be called the bimanual method, one finger in the bladder and the other hand upon the outside. From this point on, however, the operative attack should vary according to the position of the pouch.

I. Diverticula on the Posterior or Lateral Wall not in Relation to the Ureteral Orifice

In these cases the position of the ureter and the consequent possibility of wounding it need not be considered and the following technique has served us well: The bladder having been opened and the finger introduced into the pouch, the bladder wall is incised freely down to the neck of the pouch, which is then circumscribed, a clamp applied to the pouch and the rest of the dissection rapidly finished. The incision in the bladder wall, though long, has not seemed to me objectionable and it greatly facilitates speed and avoids unnecessary trauma to the surrounding tissues.

II. Diverticula Situated in the Neighborhood of the Ureteral Orifices

Under these circumstances the avoidance of unnecessary damage to the ureter is of prime importance and its identification, unless aided by artificial means, is a matter of no small difficulty and considerably lengthens the time of operation. We have believed it best to isolate the ureter on one or both sides, according as the pouch was unilateral or bilateral, in the neighborhood of the iliac vessels. The ureter is then clamped lightly with a Crile clamp to prevent wound contamination by the urine, opened and a good sized gum-elastic bougie passed to the bladder and left in place. This ma-

neuver renders the identification of the ureter easy at all times and removes the danger of its accidental incision or division. The difficulty next met with is the separation of the pouch from the surrounding tissues, to which it is generally firmly welded. It has generally seemed unwise to split the bladder wall down to the orifice as is done in the posterior or high lateral-wall cases, and particularly is this undesirable in bilateral diverticula. The separation of the sac is in some cases much facilitated by packing it with gauze, thus giving a firm mass on which to dissect and rendering the identification of the tissue easy. Our greatest difficulty has lain in the fact that these pouches are so surrounded by scar tissue and glued to everything in the neighborhood that it is by no means easy to distinguish them from other important structures, as for instance the rectum. After the pouch has been thoroughly freed (it generally must be done with the scissors and cannot be freed by blunt dissection) it may be drawn upward by means of a clamp and the neck of the sac freely divided. The wound thus left can generally be closed without difficulty, though in the cases situated below the ureteral orifice the adherence of the bladder wall to surrounding structures sometimes makes closure without tension difficult.

The hardest problem lies in those cases in which the orifice of the ureter has been drawn upward into the diverticulum. For these, Young some years ago suggested a technique which involved the leaving of a flap of bladder wall around the ureteral orifice, the rest of the sac having been excised. This flap was to be sutured into the defect into the bladder wall. This technique is ideal and I am glad to be able to report that I have recently carried it out without serious technical difficulties and I believe with a good result. This flap should have its base toward the median line, not only because of the probably better blood supply, but because in the majority of cases these sacs lie above, below or to the outer side and much more rarely to the inner side of the ureter. From these anatomical peculiarities it follows that the ureter naturally

tends to follow the inner side of the sac. Where this technique is to be carried out it is essential to have a guide in the ureter but, as I have already pointed out, I believe this to be desirable in any case. Where the conditions are such that the technique of Young is impracticable and the ureter yet enters into the diverticulum, it must of course be divided and reimplanted into the bladder wall at a point which can be reached wholly without tension. We have come to believe, largely by experience in other conditions, particularly cancer of the bladder, that the most successful implantations of the ureter are accomplished by leaving a half inch or more of the end of the ureter projecting into the bladder. The intent of this is to enable the ureter to accommodate itself to the altering conditions of the bladder and finally come to rest without tension. We have not believed that it was desirable to attach the ureter to the bladder wall by many sutures and have rarely used more than two for this purpose.

Drainage of the Ureter

The only further question is as to the desirability of providing drainage for these dilated and necessarily more or less abused ureters. In two or three cases we have placed a good sized soft rubber catheter in the ureter, extending upward nearly to the renal pelvis and have brought this catheter out through the suprapubic wound. The smooth convalescence of these patients suggests that this may be a desirable maneuver and in a considerable number of cases in which constant drainage of the ureter has been employed for other reasons, no undesirable results have been seen. This I believe to be better technique than to drain the ureter through the incision made for the insertion of the guide bougie and this incision we have always closed tightly with two layers, a technique which the dilatation of the ureter will always permit.

In conclusion I desire only to emphasize what seem to me

the important points of the operative treatment of these cases: Free bladder incisions carried down to the orifice of the sac in the more accessible cases. The opening of the ureter and the placing of a guide bougie in all cases where the diverticulum is in the neighborhood of a ureteral orifice, and finally, the constant drainage of the ureter for some days in those cases in which resection has been necessary or the flap operation of Young has been possible.

DISCUSSION

Dr. J. Bentley Squier, of New York: I should like to say just a word about a case, which I have recently operated on, of a very extensive diverticulum of the bladder—rather more extensive than any shown in these pictures. I am speaking of it principally for the reason that it was one of those cases where although I started to operate radically, I had to operate more or less conservatively. The patient, a gentleman forty-seven years of age, had been under treatment for cystitis of colon bacillus origin for three months, without cystoscopic examination, though not under my care. The cystoscope revealed the opening of a diverticulum situated about an inch above the ureteral orifice on the left side. It extended behind the bladder, way round from the base of the prostate up over the top of the bladder. A radiograph of the bladder injected with argyrol, gave a beautiful demonstration of this tremendous diverticulum, which held from eighteen to twenty ounces of urine. The patient went to operation. I told him that in all probability it would be possible to excise this secondary bladder, although I realized that its extent was so enormous that it might be a difficult thing to do. At operation we found so much pericystitis that it was impossible to separate the diverticulum from the rectum. So what I did was to take a pair of intestinal anastomosis forceps and run one on each side, clamping the posterior wall of the bladder to the anterior wall of the diverticulum. I then divided the septum between the two clamps and sutured both cut edges. I sutured it in the manner that has been employed for hemorrhoids. I was afraid that if I simply divided the septum, as soon as the man was put back to bed a part of it would grow together again, and I wanted to be sure it would not. At the same time, this puckering stitch obliterated to a certain extent this cul-de-sac behind the prostate. After I had done that on either side I removed the portion of the diverticulum which came over the fundus of the bladder. The bladder was then closed suprapubically, with a very small drain. I had opened the peritoneal cavity, because in taking off this diverticulum it was also adherent to the peritoneum. I sutured up the peritoneum first, and then closed the bladder.

That case was operated more or less conservatively. The operation was done three months ago. The patient made an uneventful convalescence. I examined him about a week ago, and at that time this cystitis

which he had had for a period of three months before had improved to such an extent that his urine was clear, and there were comparatively few pus cells in it. He emptied his bladder within one ounce. That is the result of a conservative operation. I feel that if I had tried to remove that tremendous diverticulum I would have destroyed his life. While I agree with Dr. Cabot, I feel that occasionally you will get one of these brilliant results following a more conservative procedure.

Dr. William E. Lower, of Cleveland: I was very much interested in Dr. Cabot's paper, as this subject has particular interest for me. Nine cases of diverticula of the bladder have come under my observation. As to the exact cause of the diverticula, I still feel somewhat at variance with Dr. Cabot. In practically all of the cases I have seen I could demonstrate some obstruction to the urethral outlet. In the only case of a woman that I have seen there was a urethral caruncle which so pained the patient every time she voided that she refrained from emptying her bladder as often as she should and consequently had more or less retention of urine.

I believe diverticula occur more frequently than has generally been supposed. The method of diagnosing their exact location, number and extent by injecting collargol and then taking an X-ray picture has aided very materially in working out the best method of attack. Shallow diverticula with very wide orifices do not necessarily need to be resected. Such empty themselves. But those with very narrow orifices and very large sacs, as is the case with most of them, require more radical treatment.

As to the method of removing diverticula, it occured to me some time ago that their removal would be greatly facilitated if these sacs could be converted into solid tumors, so I packed them with narrow strips of gauze. At that time I had not noticed the recommendation which Dr. Cabot has made here and which he made in 1912. I found that the tightest possible packing of these diverticula very materially aided in their removal. In one of my cases there were five diverticula in the same individual, four of which I removed by this method. After packing the sacs I found it very simple to dissect down behind the bladder and shell these out. There is a division point in the adhesions at which, if it can be detected, separation is comparatively easy.

If during the dissection one cuts through the wall of the diverticulum, he comes upon the gauze, while if in such a case the sac be collapsed it is extremely difficult to separate it. I believe, therefore,

that the advantage of packing the sacs is greater even than Dr. Cabot has led you to believe. Latterly I have taken post-operative X-ray collargol pictures and in this way have been able to demonstrate very definitely how completely I had removed the diverticula.

All of the cases I have seen came to consult me because of pus in the urine and cystitis. Two of the cases were postmen who were in the habit of holding the urine a long time. The oldest case was a man seventy-eight years old with a very large prostate—another was sixty years old with prostatic obstruction, and in that case removal of the prostate and the diverticulum relieved him. I know of no non-operative treatment which will cure this class of cases.

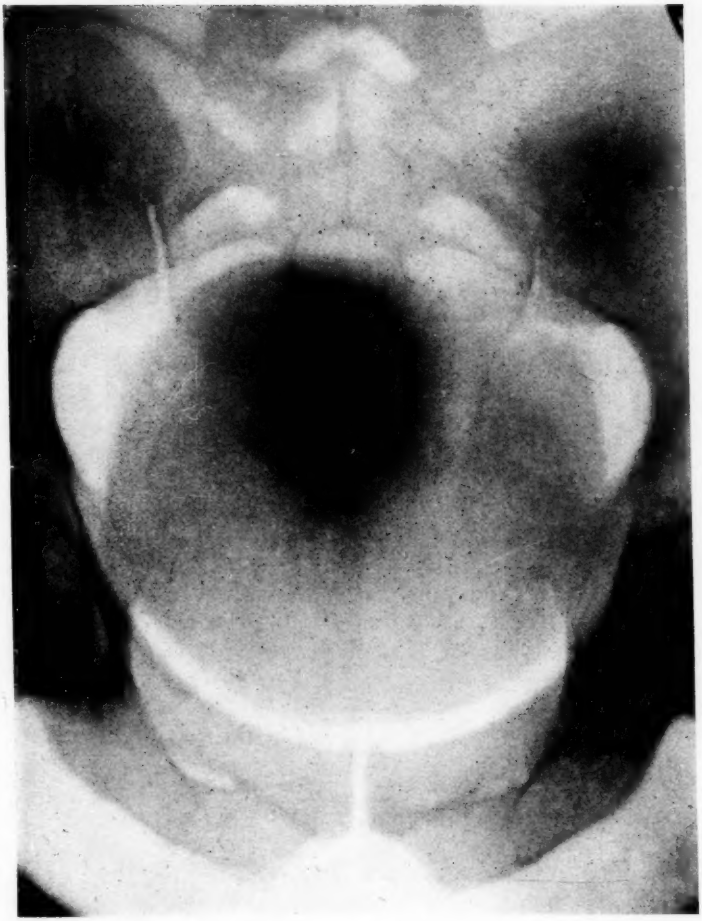
Dr. James Pedersen, of New York: I have under observation a case of diverticulum of the bladder in a man 33 years old. I think it bears out Dr. Cabot's opinion that diverticula are congenital, that they remain quiescent for an indefinite length of time, and manifest themselves only after infection. Though the patient in question has had a stricture which, judging from the history, was fairly tight, the bladder had not been subjected to straining efforts, as was proven by the absence of trabeculae. The diverticulum, therefore, cannot be regarded as secondary to the impediment caused by the stricture. Diverticula are so seldom associated with even the marked urinary obstruction of enlarged prostate as to make it highly improbable that they are acquired.

Internal urethrotomy was performed some months ago at a suburban hospital, and the moderate infection still present must have occurred at that time. Nevertheless, the patient did not begin to complain of bladder symptoms until a week or two before he was referred to me. The bladder symptoms have not been very distinctive; the chief one has been a dull pain in the lower left quadrant of the abdomen. This led his physician to suspect a renal or ureteral stone.

On examination, sixteen ounces of residual urine were found. Cystoscopy showed the entrance to a diverticulum, situated in about the same locality as Dr. Squier described in his case,—to the outer side and a little above the left ureteral orifice. The ureters were catheterized at the same sitting; the kidney function proved good, and the urine from the kidneys free from pus.

The first radiograph (Plate V), taken when the bladder was in full distension with a 15% solution of argyrol, did not show clearly the separation between the bladder and the diverticulum. A second ratiograph (Plate VI) was therefore made after the patient had voided an

PLATE V.—Presented by DR. JAMES PEDERSEN



unmeasured quantity of the argyrol solution. This gave the clear differentiation to be seen in the accompanying print. The patient will enter the hospital shortly for operation.

DR. J. BENTLEY SQUIER, of New York: Dr. Cabot, in your experience don't you feel that in these diverticula which have their origin in the neighborhood of the ureteral orifice when the orifice opens into the diverticulum it is pretty apt to be so because as the diverticulum has enlarged it has pulled the ureter around into it?

DR. CABOT: Surely.

DR. SQUIER: Well, then, why isn't it perfectly feasible in operating on these cases simply to leave a band of tissue there and not transplant the ureter? You will find in most of them that the ureteral orifice will be just in the opening of the diverticulum, and in most of them when situated there it is perfectly possible to leave a little tissue around there without making any resection of the ureter.

DR. CABOT: That is the principle of Young's technique.

DR. LOUIS E. SCHMIDT, of Chicago: I believe the monograph by Englisch, written twenty-five years ago, settled the question as to the congenital or acquired origin of diverticula. I think that upholds what Dr. Cabot has said.

I would like to say a word as to operative interference in diverticulum of the bladder. I operated on a patent urachus some time ago, which is nothing more than a diverticulum. I opened the abdominal cavity and then clamped off the base with an ordinary stomach clamp, and also a little higher up, cut about three-quarters of an inch from the lower clamp, and then sewed up the mucous membrane with a running suture, then the muscular layer and then the peritoneal covering. I should think that same method could be used elsewhere provided you could enucleate the diverticulum, and if it happens to be covered with peritoneum, the same procedure that I have mentioned. It is a method without opening the bladder, and it has some advantages over other methods.

DR. GEORGE S. WHITESIDE, of Portland, Ore.: I wish to differ with Dr. Cabot as to the congenital origin of these diverticula. If they are congenital, why don't we find this class that he speaks of, without symptoms, in young people when cystoscoping them for other purposes? Perhaps you people do. I have cystoscoped a good many normal bladders for my own and for others' experience in free clinics, and I have never found any of these cases that I can recall. It seems to me I

would have found them oftentimes if they were always there and were going to make trouble later. Also none of these reported cases, as I understand it, are in young people, but in persons approaching middle life, or later.

Dr. Cabot: Some of them are nineteen.

Dr. Whiteside: I don't get the "nineteen." Nineteen years without any symptoms, and then suddenly beginning to have them—that seems rather extraordinary, doesn't it?

Dr. Cabot: The symptoms are not sudden.

Dr. Whiteside: We all have seen cases where there is trabeculation of the bladder without obstruction, and whatever the reason for that may be, whether it is an infection of the bladder that causes frequency of micturition and straining, or whatever it is that causes the apparent hypertrophy of the muscle bundles, it seems to me a better explanation of these diverticula is Casper's idea that they are herniae between the muscular fibers rather than sacs that have always been there.

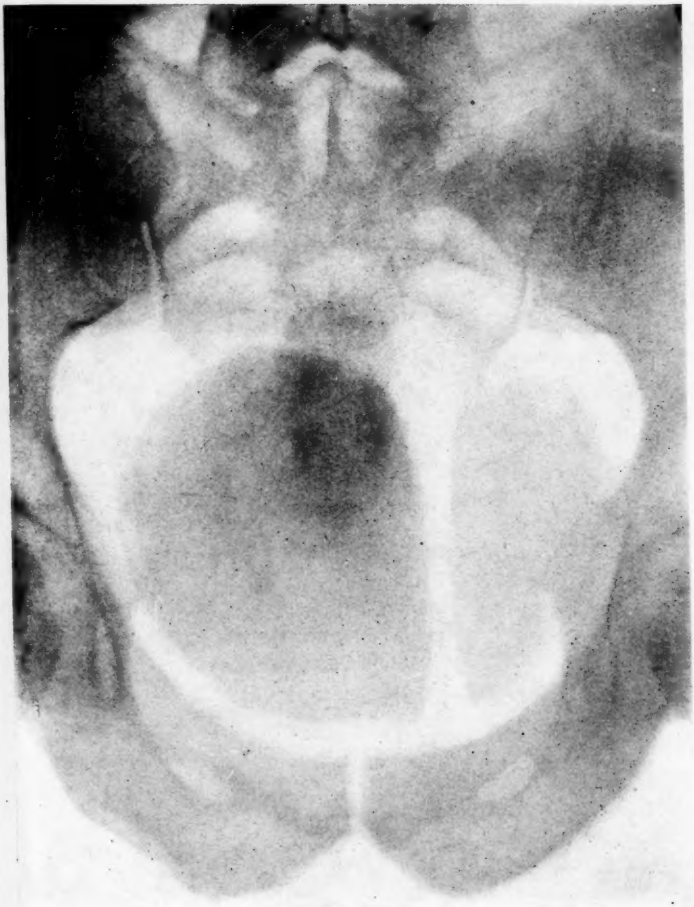
In relation to putting a catheter into the ureter through a hole from above, as Dr. Cabot advises, as a guide, I had an unfortunate experience with that procedure which has made me feel that I don't like to do it. It was an old man with a stone in the ureter at the place where it crosses the iliac vessels. I never got that fistula closed, and it was very troublesome until nephrectomy.

I have had the same difficulty that Dr. Squier has had in separating these sacs from the rectum or other viscera, and in two cases I have simply cut them off and left them in, and I cannot see that they do any harm. I left the sac in and drained it, and it seemed to close by granulation.

Dr. John R. Caulk, of St. Louis: While I believe that many diverticula may be acquired, still most that I have seen recently have convinced me of their congenital origin. A young girl with a ureterovesical cyst, who was about eighteen, had plus her ureterovesical cyst a urachal cyst and a diverticulum, with no obstruction, with a perfectly clean, untrabeculated bladder.

I want to speak of the diagnosis of the extent and size of the diverticulum. I have a case now under observation of a man who came complaining of slight urinary distress and backache. They were evidently secondary to his prostatic and vesicle condition, and I happened to strike this diverticulum accidentally. To determine the size of the diverticulum I filled his bladder with collargol, but did not get a good picture.

PLATE VI.—Presented by Dr. James Pedersen



I have obtained good pictures, but in this case there was a narrow neck situated rather high above the right ureteral orifice, and I was unable to get a good picture. But in this case I got an excellent notion of the size of the diverticulum by passing a soft X-ray catheter and then taking an X-ray with the bladder filled with collargol.

Dr. A. L. Chute, of Boston: I fear that in the paper* Dr. Cabot referred to I did not make myself perfectly clear. I intended to say there was a congenital predisposition in these cases of diverticulum, in the way of a little pit or dimple in the bladder wall, and that the location of the diverticulum depended upon the situation of this predisposing lesion. These diverticula appear in the general region of the ureters most often, because this is the place where these little congenital defects are most frequently seen. It seems to me that to this extent you have in almost all of these cases a congenital element which acts as a predisposing factor.

I am going to hold out for a certain amount of obstruction as the determining cause in most of these cases. I can at once think of six cases in which there was a definite and very evident cause for back pressure. Two of them had movable prostatic lobes, one in a man in the early forties, the other in a man of sixty. Another patient had a tight stricture, while a fourth had a gonorrhoeal prostatitis; the two others appeared in men with obstruction due to ordinary hypertrophic prostates. It seems to me that these, and even lesser things, may be the cause of increased pressure. I had not thought of the explanation that Dr. Lower mentioned of a man who had to hold his urine a long while. Chronic or acute cystitis might well be the determining cause in cases where the congenital defect extends pretty well through the muscular layer of the bladder wall. If it goes through the muscle wall you may get a hernial protrusion with very little back pressure.

Dr. E. Wood Ruggles, of Rochester, N. Y.: I had an interesting case six or eight years ago of a gentleman, forty-eight or fifty years old, who had a severe cytitis. I cystoscoped him and found a diverticulum with a stone almost filling it. I should think the orifice was about an inch in diameter, and from the curvature of the stone which completely filled it and projected into the bladder, I should say it was an inch and three quarters in diameter. I told him it could not be crushed with any success and advised removal of the diverticulum. He refused opera-

*Two Cases of Diverticulum of the Bladder Treated by Operation. Trans. American Urological Association, 1912, Vol. VI, p. 64.

tion and another surgeon crushed the stone. I heard about him four months later, and the cystitis was as bad as ever.

Although it has no direct connection with the subject of the paper, I would like to narrate a case which presented some features similar to this emptying of the urine contained in the diverticulum under voluntary contraction of the bladder. This patient had great difficulty in starting his urine and the additional symptom that on trying to forcibly express it, the urine would suddenly cease flowing. This led me to attribute it to a ball-valve condition at the neck of the bladder. A large soft rubber catheter passed easily.

I then filled the bladder and found that so long as he did not try to force the urine it passed from the catheter easily. The instant he tried to expel the urine forcibly the flow ceased. I introduced a French web catheter and found that the flow then increased upon expulsive efforts, thus proving that the stoppage was due to muscular contraction.

I cystoscoped him and found nothing abnormal about the bladder. The urethral mouth was normal, except that at the bottom it was congested. This made me think that there might be verumontanum trouble. Through the urethroscope I found a very pronounced inflammation of the verumontanum, which I treated, and he came in a week later saying that the symptom had been decidedly better. I treated him again, and have not seen him since. It seems evident that the irritable condition of the verumontanum produced a reflex contraction of the urethra upon any attempt at forcible ejaculation of the urine.

Dr. Charles H. Chetwood, of New York: It seems to me that in discussing the etiology of these cases the question is not so much whether any of them occur with the existence of bladder obstruction, because such are self-evident, but whether any of these cases do occur where there is no bladder obstruction, where the important evidence is residual urine or beginning atony of the bladder, and as in the history in some of these cases none of these symptoms exist, it would lead us to the acceptance of a congenital origin of such of them.

Dr. Edward L. Keyes, Jr., of New York: I have seen at least one case in a man in his thirties of diverticulum high up in his bladder holding some six or eight ounces in whom I could identify no urethral obstruction whatsoever.

I should also like to ask whether any one has identified the presence of diverticulum in a bladder the urine from which did not contain pus? As far as I know, all of the cases that have been reported have

been with purulent urine. It would be interesting to know whether a diverticulum ever does give symptoms until it becomes infected.

Dr. R. F. O'Neil, of Boston: I should like to add a word to what Dr. Squier has said in regard to turning a flap containing the ureter into the diverticular orifice. This was done in one of Dr. Cabot's operations in which I took part and it worked very nicely, filling the orifice without tension and without difficulty.

Dr. Pedersen: If diverticula are acquired, why is it that they are not encountered more frequently, considering the very many cases of urinary obstruction that exist when due to stricture and to prostatic enlargement are included?

Dr. Hagner: I haven't much to say about my own case except one or two little points that have been brought out in the discussion. It has always seemed to me that these cases were congenital, or that there was some congenital defect, especially in the large diverticula. This man was seventy-six years old, and had no trouble until six weeks before I saw him. He gave a history of having the ability (all his life) of holding his urine a long time; from this it would appear that the diverticulum had been there probably all his life. He had an infection at the time his prostate was removed, but now he has a very slight infection, although he has sixteen ounces of residual urine.

Dr. Cabot: In discussing these cases of diverticula I think we want clearly to distinguish between diverticulum and patent urachus. They are two distinct conditions, and bear no relation to each other. I have not included any cases of patent urachus.

On the question of symptoms I did not make myself clear. In case 1 symptoms began at the age of twenty-five; case 2, at the age of twenty-eight; case 3, at the age of twenty-six; case 4, at the age of twenty-seven; case 5, at the age of fifty-eight; case 6, at the age of forty-two; case 7, at the age of twenty-one; case 8, at the age of forty-one; case 9, at the age of thirty-eight; case 10, probably about sixty. They will figure out an average age of about thirty-seven, and that in spite of putting in some of these old men with symptoms for many years. We do not know anything about the origin or the real beginning of their symptoms. The average age of these people is less than the time beginning of the ordinary obstructive lesions of the prostate. In the other cases there was no stricture, no conditions in the prostate which I believe to be abnormal. I can find no evidence whatever of a lesion sufficient to push out the coats of the bladder. These are not herniae

of the mucous membrane. They are covered by all the coats of the bladder, and in the case of the high ones, by the peritoneum. They are not simply a bulging between the muscle bundles; they are covered by the muscle itself.

DR. WHITESIDE: There is more than one layer of muscle.

DR. CABOT: There are several.

DR. WHITESIDE: Do all the muscle layers come on the outside of these?

DR. CABOT: So reported by the pathologist.

DR. WHITESIDE: This average you give as thirty-six?

DR. CABOT: Yes; rather early for prostatic obstruction.

In regard to the opening of the ureter, I do not believe opening the ureter is objectionable from the fear of ureteral fistula. Ureteral fistula means obstruction below. If there is no obstruction below ureteral fistula will not occur. We have opened the ureter now in a very large number of cases and have had no cases of ureteral fistula except those where there was obstruction below. In two cases of tight strictures we did not efficiently remove the strictures and fistulae resulted. Subsequent removal of the stricture in one case and nephrectomy in another were necessary. A resulting fistula means that we have not gotten rid of the obstruction.

We have not been able to get good pictures with collargol. I think our technique has been faulty. Such a picture as Dr. Pedersen has shown has not been obtained in our cases. In the cases with residual urine the amount of the residual was a very good measure of the capacity of the diverticula. Of course, if you have only one diverticulum the amount of residual urine equals the size of the diverticulum. In prostatic hypertrophy you have no guide.

Five or six of these cases had stones in the diverticula, and I believe this to be common where the diverticula have existed for a long time and where there has been a marked chronic cystitis or diverticulitis. I think they are secondary stones, similar to those formed in the bladder behind an obstructing prostate.

The separation of these diverticula from the surrounding structures is at times very difficult. Packing with gauze, which we referred to several years ago, is helpful, but in the cases that are glued on to the rectum, the iliac vessels, and at times to the pubic rami they have to be dissected off with scissors and knife, and the gauze, though helpful, sometimes is not more helpful than the finger. Often one has to put

the finger into these sacs and dissect right on the finger to get them off, and when one gets through the pelvic structures are exposed much as are the structures of the neck after gland operations. I know of no more difficult situation in which to dissect, or one which will cause you more trouble than the ones which lie between the rectum, the pubic bones and the base of the bladder. On the other hand, I have been impressed by the fact that these patients stand the operation very well, probably because we are not dealing with structures particularly prone to produce shock. Some of the operations have been very long, but we have not had any mortality in this small series of cases. Many of these cases were poor risks, and yet they have stood the operation well. The more I have done it, the more I have been willing to undertake obviously tedious and difficult operations. But, on the other hand, such an operation as Dr. Squier suggests seems to me a perfectly efficient and satisfactory compromise in cases in which you feel as the operation goes on that some form of compromise should be made. I should be unwilling to compromise in any of these cases in which success was probable.

OBSERVATIONS ON THE SEMINAL VESICLES

By J. Dellinger Barney, M. D., of Boston

THE importance of the seminal vesicle from a clinical and pathological standpoint has become great in recent years. In the not distant future we can be more dogmatic about certain aspects of the question. At present we may be said to be feeling our way with X-ray, knife and microscope.

The trails first blazed by our colleagues, Fuller and Belfield, have inspired others to careful investigations which have yielded interesting and important results. The physiology of the seminal vesicle is now fairly well known, thanks to the work of Walker, Namba, Furbringer, Huet, Steinach, de Bonis and others. It has long been known that the seminal vesicle acts as a reservoir or resting place for spermatozoa as they come up from the testicle. It is a matter of ordinary clinical experience that if the normal vesicles are carefully massaged, myriads of active spermatozoa may be expressed. Furbringer in 60 cadavers found spermatozoa in the seminal vesicles in 80 per cent; whereas Schlemmer found them present in 89 per cent of 156 autopsy subjects.

That the seminal vesicle has a secretion of its own and that this secretion is almost a *sine qua non* of procreation has been shown by the work of Steinach, and more recently by Walker. Both these investigators, working with white rats, showed that extirpation of the seminal vesicles alone caused a marked decrease in the high percentage of fertility common to these animals. Walker further showed that the secretion of the seminal vesicles of the rat is a very complex substance, that it contains histones and some of the higher

proteins, and that the spermatozoa of these animals are immediately clumped when mixed with the vesicular secretions. He also demonstrated that this phenomenon does not occur in guinea-pigs.

Huet's experimental work has shown not only that bacteria are present in the secretion of the seminal vesicles of healthy animals, but also that in animals dying of acute sepsis the specific organism is to be found in this secretion. The seminal vesicle may thus be looked upon not only as a reservoir for spermatozoa, and a secretory organ, but also as an excretory organ. If we are to regard as sound Belfield's conclusions that the testicle and epididymis are organs of excretion, corresponding to the kidney and its ureter, then we must regard the seminal vesicle in the same light, for after all it is but a complicated offshoot of the vas deferens, essentially the same, from an embryological viewpoint, as the latter structure and the epididymis.

The experimental investigations in animals by de Bonis throws much light on the activities of the seminal vesicle and his observations agree in the main with what clinical data we have. He finds that the vesicles develop rapidly at the age of puberty, decrease in activity during hibernation, secrete freely during the rutting season and after castration. The secretion of these organs arises from the activity of the cells lining their cavities. The process of secretion begins in the nucleus while the cytoplasm swells with liquid. The granules and plasmosomes pass into the cytoplasm advancing progressively toward the border of the cells and by rupture this secretion passes into the glandular acini. At different ages the cells present a different appearance. At birth the gladular structure is very rudimentary, while at sexual maturity the secretory activity attains its maximum. After castration there is a notable decrease in glandular secretion even if complete atrophy does not occur, and it has been shown that the injection of testicular extract in cas-

trated animals does not at all excite the secretion of the seminal vesicles.

The extensive investigations of Namba have shown also certain pigment granules of the nature of lipoids, in the muscle and epithelial cells. What their function is seems to be unsettled. Namba has also shown the presence of large amounts of elastic tissue in the seminal vesicles. These fibres are always to be found in the basement membrane surrounding the glandular acini and to a certain extent it runs up into the numerous villi which protrude into the glandular cavities.

So much for the work of others. The valuable operative lessons taught by Fuller, and the interesting clinical observations of Belfield are too well known to be dwelt upon here. Our own investigations have substantiated in man many of the findings of others in animals as well as in the human. In addition we have brought to light certain facts not hitherto recorded.

The presence of elastic tissue in the normal vesicle has been readily shown. It is relatively large in amount, situated almost wholly in the subepithelial tissues with fairly constant prolongation into the villi of the gland cavities, and in certain normal cases has been seen in minute quantities scattered irregularly through the muscular walls. In disease, in the presence of considerable sclerosis, this elastic tissue seems to be generally decreased in amount and is irregular in its distribution. On the other hand certain secretions have shown here and there bundles of elastic fibres far greater in size than any which we have seen in a normal specimen.

The connective tissue of the seminal vesicle has not received much attention by others. We have studied it carefully in health and disease. In infants it is relatively scanty but like the elastic fibres it seems to lie largely in the subepithelial tissues, although to a greater extent than elastic tissue, it permeates the muscular walls. In the normal adult

vesicle its presence is well marked and it is to be seen in some instances, like elastic tissue, running into the glandular villi. Infection of the seminal vesicle seems to result invariably in the deposit of very large amounts of connective tissue. In some specimens, with longstanding infection, the walls are composed of but little else than dense fibrous tissue with here and there a few atrophied muscle bundles. It is the contraction of this scar tissue and the subsequent obliteration of all or portions of the glandular cavity which we find in some of the specimens with a history of infection dating back for many years.

Dissection of numerous specimens at the autopsy table, together with our operative experience has shown that this fibrous tissue does not confine itself alone to the body of the seminal vesicle. In many of the specimens we have found these organs embedded in a dense cake or plastic exudate, always most marked at the prostatic border and obliterating entirely the sulcus between prostate and vesicles and between the vesicles themselves. It is this deposit of scar tissue which we feel in the living by rectum and it is this which at operation often makes the exposure, to say nothing of the excision, of the vesicles a matter of exceeding difficulty. It is this which also prevents the removal of the vesicle alone apart from the vas deferens. Separation of these structures under these conditions would be an impossible procedure.

We have nowhere seen mentioned the finding of nerve fibres in or near the seminal vesicles. It is well known that these organs are supplied and surrounded by fibres of the sympathetic nervous system. We have been fortunate enough to meet with these nerve filaments in cross section in one or two specimens, demonstrating very well not only their presence and size but also their situation in the tissues immediately surrounding the vesicle.

Many observers have reported their bacteriological findings from the contents of the vesicular cavities. Picker has

shown the gonococcus, Voelcker the pneumococcus and others have shown staphylococci and streptococci. We have seen no mention made of the finding of the Bacillus coli but we are convinced of its presence in certain cases.

In several autopsy specimens and at operation in a few cases we have taken cultures of the vesicular contents. All have been sterile. Finally it occured to us that the situation in the seminal vesicle might be like that in the vermiform appendix, recently described by Rosenow of Chicago. This investigator has found the streptococcus in the walls of the appendix, in the submucous tissues, and believes that appendicitis is due in certain cases at least to this organism. In view of this work we have submitted the seminal vesicle from one patient with acute joints and a fairly recent gonorrhœa to such an examination. This examination was kindly made for us by Dr. Hugh P. Greeley of Boston who has done a considerable amount of work along this line and to whom we acknowledge our gratitude. Dr. Greeley found in the specimen mentioned an intramural organism, an unknown bacillus, corresponding in many ways to the bacillus of Ducrey. Incidentally the culture of the secretion from the vesicle was sterile. What the organism is or what part it plays in the lesion of the vesicle we are unable to say. It is at least an interesting discovery and one that should lead to exhaustive investigation of the same nature. If substantiated it may clear up some of our problems of infection.

Testut has observed clinically, and de Bonis experimentally, that after castration the vesicle atrophies. We can bear out their statement by a case seen post-mortem. The right testicle had been removed for some unknown cause years before. At autopsy the seminal vesicle on that side was nowhere to be found, the site being occupied by a small band of fibrous tissue. The vesicle on the opposite side was present and normal in every respect.

In connection with this it is of interest to note the findings in a case operated upon by Dr. Cabot. There was a well

developed arthritis of long standing, an acute gonorrhœal epididymitis on one side and an undescended testicle on the other. The vesicles were exposed in the usual manner and that on the side of the undescended testicle was quite as well developed, also as badly diseased as its fellow. Cryptorchidism therefore would seem to be without influence on the seminal vesicle.

In the belief that the seminal vesicle is an excretory organ we have searched not only in its walls but in its secretion for the spirochæta pallida in two cases of congenital syphilis. Our efforts have proved futile even though in both these cases the spirochæta was easily found in almost every other organ of the body. We believe our failure has been one of technic and further attempts are to be made. If the spirochæta can be demonstrated in the seminal vesicle of an individual with uncured syphilis may it not explain, at least in part, the transmission to the offspring of the disease without obvious infection of the mother?

One of our chief objects has been to establish a more accurate method of diagnosis than simple rectal examination. In many cases this will show the dilated, indurated, tender vesicles which are so characteristic. In other patients no definite abnormality of the vesicles can be made out owing perhaps to the deposit of an extensive cake of plastic exudate overlying and obliterating prostate and vesicles; perhaps to the small size or difficulty in reaching the organs in question. Massage may or may not improve the arthritis and the general toxæmia from which the patient suffers.

How are we to know whether the focus of infection lies in the seminal vesicle? It is our belief that the X-ray will solve at least some of these difficult problems. In order to know what is abnormal we must know the normal and but little work has been done along this line.

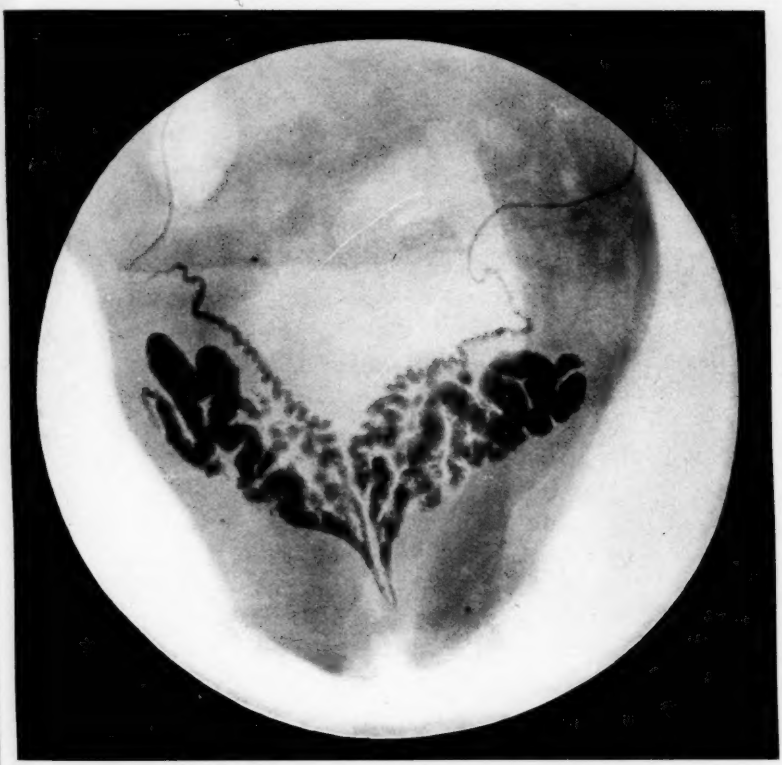
We have removed the organs from several autopsy cases, injected the seminal vesicles with collargol, taken a radiogram, noted carefully the appearence of the shadow and

have compared the picture with the microscopic findings. Where the microscopic sections of these organs were normal there was no gross evidence of disease. One such specimen obtained post-mortem shows a very different picture. Whereas in the normal cases one can readily follow the tortuous tube of the seminal vesicle throughout most of its extent (Plate VII), in the pathological specimen one sees a distinct dilatation of the cavity. So extensive is this that we see not the various convolutions of the lumen but only a dense mass of injection fluid due to the overlaping of one dilated convolution upon the other (Plate VIII). Microscopically these vesicles were in a state of subacute inflammation, the walls were thickened and the cavities much dilated.

The X-ray shows also very prettily in the case of a child three months old the simplicity of the cavity of the seminal vesicle, the almost complete absence of the ejaculatory ducts and the small size of the organs.

We have also injected one or both vesicles in many cases before or in anticipation of operation. In cases which unquestionably needed operation we have adopted this procedure in the hope of increasing our knowledge of the appearances of the pathological vesicles; in cases of doubt we have injected the organs in the hope of eliminating that doubt by the picture obtained. In some respects these clinical radiograms have compared closely with those made on autopsy specimens, but in the former series we have injected no case known to be normal. Thus in the living it is not feasible to make the comparisons which are possible in the dead. Furthermore owing to the difficulties of such an injection in the living and on account of the presence of other intervening substances and organs the best of radiograms is poor as compared with those on the cadaver. However, on the whole, the results are quite satisfactory and thus far have pretty constantly borne out our belief that it is possible

PLATE VII.—Presented by Dr. J. D. Barney



Normal seminal vesicles, with vas and ampulla, removed at autopsy. Injected with collargol. X ray.

Convolutions of vesicles easily identified. Note the multiplicity of diverticula of vesicles and ampulla.

to make a diagnosis of a diseased vesicle by a collargol radiograph.

One case so injected had all the evidences of disease of the seminal vesicle except that these organs did not feel pathological. The X-ray shows a shadow quite unlike those of definitely diseased organs and subsequent events have shown that the pain from which the patient suffered was due, not to vesiculitis, but to chronic appendicitis with adhesions.

A valuable lesson has been taught us by the X-ray pictures. In those of the autopsy specimens especially, and to some extent in the clinical cases, one can see the extraordinary complexity, not only of the seminal vesicle but also of the vas deferens at its ampulla. Some of the seminal vesicles have budding from their walls diverticula of considerable size, connected with the main organ by a narrow tortuous neck (Plate VII), and found microscopically to present the same picture, whether diseased or healthy, as the seminal vesicle itself. Examination of the ampulla of the vas deferens shows an even greater complexity of lumen and a larger number of diverticula (Plates VII and VIII). In the presence of these peculiarities of each structure is it not easy to see why infection once started in vas or vesicle is so persistent, why massage may be so inadequate, and why excision, rather than drainage by multiple incisions, should be the operation of choice? How is it possible in the presence of some of these large and unsuspected diverticula to effect a cure by such methods? An infected diverticulum, left untouched, as it surely would be by simple incision of the vesicle, is perfectly capable of carrying on the work of the vesicle itself in the production of symptoms.

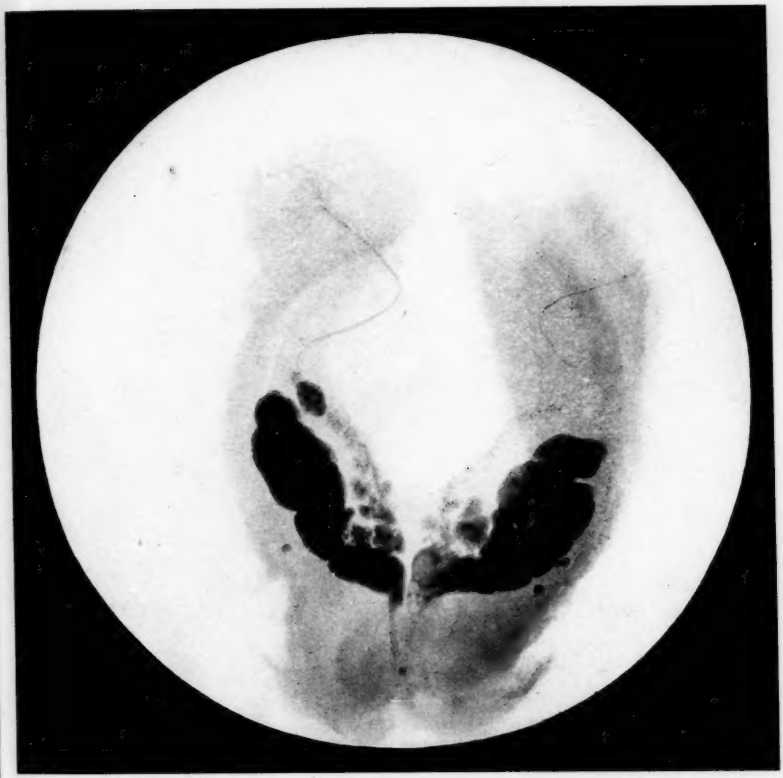
To sum up, our investigations with the microscope, the knife and the X-ray have shown certain definite facts about the seminal vesicles. We have found that if one organ is diseased so also is its fellow; that during the early stages of infec-

tion of the vesicle, dilatation of its cavity occurs owing either to a cessation of its normal peristalsis or to an obstruction of the ejaculatory duct or to both. Later on this is followed by a deposit of more and more scar tissue in and around the vesicles until they become lost to rectal palpation and when dissected are found to be enormously thickened and with cavities shrunken and in some places almost obliterated.

But we have not yet touched upon the important question of the condition of the prostate and of the ejaculatory ducts in the presence of pathological seminal vesicles. We have every reason to assume, on clinical grounds at least, that infection and morbid changes in the latter accompany similar conditions in the former. This being the case it will explain some at least of our failures to cure chronic arthritis by vesiculotomy and even by vesiculectomy unless the prostate is also removed. We must not forget the brilliant work of the late Dr. Alexander on the pathology of the prostate, nor should we forget to apply his teachings in the cases with which we are dealing.

In closing it seems fitting to say a few words about the more or less immediate results of vesiculectomy. All of our cases have been operated upon too recently to draw definite conclusions. With one or two exceptions these patients have relapsed after operation; in some, infected joints, formerly quiescent, have exacerbated; in others fresh joints have become involved. In most, so far as we know, such reinfections have been short-lived and not serious. Such results bring up the question just touched upon of the feasibility of removing the prostate or at least draining it by free incision. They also show, statements of other operators notwithstanding, that although radical measures for the relief of vesiculitis may and undoubtedly do produce a cure in the long run, the patient's path is not at once strewn with roses.

PLATE VIII.—Presented by Dr. J. D. Barney



Pathological seminal vesicles, with vas and ampulla, removed at autopsy. Injected with collargol. X ray.

Convolutions of vesicles not made out owing to dilatation of cavities. Diverticulum of vesicle on left side, near the apex, connected with vesicle by narrow, tortuous sinus.

Microscopically these organs showed marked inflammatory changes.

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DISCUSSION

Dr. J. Bentley Squier, of New York: I am glad to take this opportunity to tell you of a few experiences which I have had with operating upon the seminal vesicles. Inspired by the enthusiasm of an ex-president of this Society and the wonderful cures which he has from time to time reported, some two years ago I thought I would try to acquire an experience of my own. Knowing even at that time, before these injected radiographs had been taken, that the seminal vesicle was not a simple hollow organ but an organ composed of a complex system of tubules, I thought if we were going to do any operative work on it we ought to devise some exposure for that organ which would allow us not only to drain the vesicle and the ampulla of the vas but some of its numerous little diverticula which harbor infection. So, working along a different line, I worked out a technique for myself, which is not at all original except as to two or three points, which allowed of a thorough exposure of these vesicles, so that they could be drained with great ease. I have operated, up to the present time, about fifty good, bad, and indifferent cases, with good, bad, and indifferent results.

Just now I believe we are only feeling our way along in this subject, and I would caution against vesiculectomy in the majority of cases at the present time, and for this reason: if as Dr. Barney says these little diverticula are quite as important as the vesicle itself—and I believe this—and they need to be opened and drained, and he feels that an incision or so in the vesicle will not be enough to empty these diverticula, and that excision is preferable—if, as he has shown, it is possible to inject these diverticula, why will they not drain through multiple punctures in various places in the vesicle? Secondly, we know that nature has a strong reparative process in any diseased organ, and while we are feeling our way along, why would it not be better to drain rather than to excise? I do not believe any of us can as yet outline the definite operative indications in the chronically inflamed vesicle.

The results of my experience have been very peculiar. In summing the work up the cases have been divided into three classes. The cases of chronic inflammation of the vesicle following a long-continued urethritis have been the ones which have been least benefited by operative treatment, the man who goes the round with the chronic drop in the morning, and associated symptoms. The element of infection in

the prostate and in the urethra no doubt in these cases is quite as important as in the vesicle.

Take the cases in which we have joint involvement. The immediate effect in those cases—and I think Dr. Barney and Dr. Cabot will bear me out in this—is little short of marvelous in a great many instances.

The question of recurrence after drainage as against recurrence after vesiculectomy I think has to be proved. I think the statements which we have heard certain men get up and make in meeting that it is a certain and sure cure of various types of rheumatism are absolutely erroneous. I do not believe it. But the immediate effect is marvelous. A man will be laid up with a number of joints, and if you drain his vesicles, inside of forty-eight hours he will be free from pain in a great many instances. Many men have not succeeded in obtaining such results because the drainage has been incomplete.

We have come across a number of unique instances. One man had spermatic colic during intercourse. Three such cases were submitted to operation, and small calculi were found in the vesicles, and the removal of these calculi caused a cessation of the symptoms. These calculi could not be palpated by rectum, because there was a large plastic exudated from perivesiculitis which masked the vesicle proper. That is another exceedingly interesting class of cases.

The point which I believe we are coming to is this: the time to drain these patients is before chronic changes have taken place, and I think the evidence will develop an opinion which will lead us to operate upon cases of acute seminal vesiculitis; the type which we now put to bed with hot irrigations, and congratulate ourselves upon the fact that the prostate has not suppurated. I think that would be the best time for operating upon the typical gonorrhoeal vesiculitis.

The main points in the technique of the operation are these: A perineal incision is made similiar to the skin incision for the exposure of the prostate. The rectum is dissected away from the prostate, and the muscle fibers between it and the urethra severed, and the prostate is thoroughly exposed. In our early work we started to use the tractor which Dr. Hugh Young showed some two years ago to pull the vesicles down, but we soon found that this instrument hampered rather than made the operation easier. The easiest way to do it is to have no instrument in the urethra or in the rectum, but to expose the prostate and then carry the blunt dissection up further behind the prostate, separating the rectum from the base of the prostate and the base of the blad-

der, then using a flexible tractor and bending it at a right angle, use it as a posterior tractor. Two heavy silk sutures are inserted between the base of the bladder and the prostate, and traction on these sutures rotates the prostate around at an angle of sixty degrees and brings the vesicles into view at the bottom of the wound. These are covered by the fascia of Desnonvillier, and this has to be cut through. As soon as it is divided the vesicles pop into view. They are easy of access, and it is very simple to drain them in the majority of cases. Every once in a while you will find a vesicle which has an extensive perivesiculitis, and these are the cases that are difficult of access, difficult to pull down, and sometimes difficult of excision.

I think the work is in its infancy, but I do hope that the members of this Society will acquire experience in work on the seminal vesicles, so that when we meet next year we can outline definite indications for operating upon them.

Dr. B. S. Barringer, of New York: An interesting point in the recurrence of these cases came to my notice some time ago. A vesiculotomy was performed for rheumatic symptoms and general pain in the prostatic region. The patient was well for three months, when everything recurred. He was operated on again by the same surgeon, was well again for three months when all symptoms recurred.

Dr. George K. Swinburne, of New York: I had at the end of last summer four cases in which the vesicles had been drained in this manner. They came into my hands with just as severe symptoms of rheumatism, which had been relieved only temporarily if at all. I have had a little experience this year that I should like to relate in regard to the use of the high frequency current and also the static wave current over the vesicles and prostate. It is oftentimes very difficult to say whether we have involvement of the vesicles when we have a prostatitis, and vice versa—at least it is for me. We often feel the vesicles and know that we empty them, and under the microscope we find dead spermatozoa mixed with pus and so on. Then, too, we can feel that the vesicles are bound down by inflammatory exudate.

One case that I had was a gentleman who had been for a long time in Baltimore. He had a colon bacillus infection of the prostate. He had never had gonorrhoea. He was urinating about every hour. He had lost twenty-five pounds in weight, and he was in a very wretched condition. He had a bacteriuria, which showed a pure culture of the colon bacillus, and he could only come once a week. I would massage

his prostate and then apply the high frequency over the prostate. I want to say, in the first place, that I had an idea that the cloudy urine might come from the kidneys, and in the very beginning of his treatment I catheterized both of the ureters and obtained a normal specimen of the urine from each kidney. But the urine as passed from the bladder was always cloudy. In six months' time he had gained twenty-five pounds in weight. His intervals had increased to about three hours, and at the end of a year he could hold his urine for six hours. He also came to me for a part of the second year, and his urine became perfectly clear. I saw him last winter, and he told me that his urine was perfectly clear. I do not know whether the colon bacillus can be still found. I think this case was due to constipation.

I had another case which I think was a gonorrhoeal prostatitis. The man had had a double epididymitis some time before. I started with him by massage and the static wave current. He got so much relief from it that he persisted in coming. I did not think he would come very long. I used to get an immense amount of material from the prostate by massage. In the beginning I used to examine it under the microscope, and it was always loaded with pus and nothing else. This went on for a year. As he used to come during my busy hour I stopped examining it under the microscope. But about two months ago I examined it again, and I got a good specimen of living spermatozoa and just a few leucocytes here and there.

Dr. Francis R. Hagner, of Washington: I should like to cite two specimens obtained at autopsy, as possibly some of these old cases may have this end result, and I would like to ask Dr. Barney if he has noted this condition. These were both old men, and in both of these cases the seminal vesicles were filled with these little calculi that Dr. Squier has spoken of, and the vas deferens on each side was a calcareous tube. The lumen was compressed, and this deposit appeared to be in the walls of the vas deferens just the same as it would be in an artery in arteriosclerosis.

Dr. Louis E. Schmidt, of Chicago: I think clinically we can group our symptoms very nicely, as far as seminal vesiculitis is concerned, but it really gives no practical information as to the operative method of procedure. Up to the present time, I have followed this rule in the treatment, that is, beyond the ordinary, regular treatment: If I can outline or feel fairly distinctly an infiltrated mass which probably corresponds with the seminal vesicle I have advised and carried out in

a number of cases drainage of the seminal vesicles, and in those cases where the vesicle appears as simply rather acutely involved, or rather recently involved, and where there is no hard infiltrate to be felt, I have drained the vesicles according to the method of Belfield, with vasostomy, and at the same time medicated the vesicles in the way he has described. I might say, however, that I am in perfect accord with what Dr. Squier has said. I do not believe what you can feel per rectum is a safe guide to the method of procedure, and the more I have seen of this work, the more I am inclined to believe that early drainage in the recent cases will probably give better results than if we wait too long.

Dr. Squier: Dr. Lower just brought this up. There is one very sad post-operative sequela. Four men have had impotence following drainage of the seminal vesicles. One was a patient sent to me who had been married last May. He acquired an acute specific urethritis five days after the wedding. He went through acute seminal vesiculitis, was laid up in bed and had hot irrigations, etc., and eventually presented himself to me with a morning drop, palpable vesicles, and he wanted to get well. He had already had treatment for a period of eight months. I told him he might try having his vesicles drained. I told him it was a new operation, and that I could not guarantee him very much. His vesicles were drained. It cleaned up his symptoms as far as the inflammation went. But he had a degree of impotence following the operation, and the impotence was of this nature: it was peculiar, because it has been of the same type in these four or five cases. They have the desire and a partial erection, but the erection does not become rigid enough to allow entrance into the vagina. They do ejaculate.

The second case was following an acute articular invasion in a young man twenty-four years of age. Following his operation he was greatly distressed because two months afterwards he was unable to have intercourse, for the same reason that I have detailed in the other case.

The third case was a married man in New York, a man who was of an extremely high nervous temperament. One of his brothers committed suicide insane, his father was insane, and his mother died insane. The man has been unable to perform coitus since operation.

The fourth case was impotent for three months. Following that he was able to perform the act, and has for the last six months.

Whether the operation has had anything to do with it or not, I do not know, but that is the story. I think it is well to bear that in mind.

Whether the excision has any more effect than the drainage, I do not know.

NOTE: July 1914. All of the above cases have regained their sexual power.

DR. WILLIAM E. LOWER, of Cleveland: Dr. Squier was good enough to come to Cleveland about a year ago and demonstrate the technic of his operation. It appealed to me. The first case thereafter in which this operation seemed indicated was a professional man. I exposed his seminal vesicles, pulled them down as has been described, and drained them. The patient's condition improved but he has been impotent ever since. So far he has not complained of this condition because he feels so much relieved of his rheumatism and although it seems to me that he is permanently disabled, he himself does not think so, as he has strong desires. I am very anxious to know if mine is an isolated case, or if impotency is a common sequence of this operation. I should like to know if any others have had similar experiences. It strikes me that this is a serious complication.

DR. SQUIER: I do not think it is going to be a hundred per cent. There were only four in about fifty-seven or fifty-eight cases.

DR. HUGH CABOT, of Boston: I think this is an extremely important matter. We have been interested in it for some years, but only in the last year have we been willing to do a radical operation. I am particularly interested in Dr. Squier's operation, because it seems likely to take the place of vasectomy, so common in the Middle West. If you could only insure the impotence of these fellows, it would be highly desirable.

It seems to me, from our small experience, that one of the things upon which we want to lay stress is the careful exclusion of the word "easy" for any stage of this performance, for it is not easy. I regard it as a highly difficult operation. The more operations one does the less difficult it becomes, but I do not believe it ever becomes anything less then a difficult operation.

Dr. Squier's amendment of Dr. Young's technique seems to me an improvement. I used Dr. Young's tractor and then adopted Dr. Squier's sutures, and I am perfectly clear that it makes the access to the field easier for me.

I can get no theoretical nor practical conviction that vesiculotomy is going to do the business. On theoretical grounds the extreme complication of these structures seems likely to leave pockets whose tendency

will be to become closed off rather than to become emptied. Clinically I have on two occasions thought I had freely exposed and emptied these vesicles, only to find later that I had left considerable portions of them behind entirely untouched. I think it is never possible to see, unless the vesicle is removed, just how much of it there is. When you have got the vesical out you can see whether it is intact, but only in this way can you be sure. We have in practice reserved this operation for cases of arthritis of a severe grade in which a severe and perhaps mutilating operation is entirely welcome to the individual. We have not applied it to acute cases. I am, however, in sympathy with Dr. Squier's and Dr. Schmidt's view, that it might be possible to do some less formidable operation on acute cases. But for chronic cases I am satisfied that it is wise to go the whole distance or leave them alone, and the whole distance I think means the removal of the entire vesicle and the lower portion of the vas upon both sides.

The benefit to our patients from these operations has not been miraculous in any case. I think they have all of them felt a little better after they got over the immediate effects of the operation. I am not sure that they have felt better than they would have felt if we had kept them in bed an equal length of time. I cannot help remembering that patients on the orthopedic service who have had to be etherized and remain in bed for other conditions, have had their rheumatism improved and remain improved for years, a fact which many men with large orthopedic experience will verify. The same has occurred after diseases of other organs, typhoid or pneumonia; and operations on other parts of the body have been followed by improvement. The connection between the cat and the tail does not seem at all clear. I am not at all sure that what we did was the cause of the improvement in the rheumatism. It is a subject about which we know too little, about which we must learn more; but we should not foster the widespread enthusiasm about operating, and above all things we should not lead anybody to believe that it is a simple procedure. I think it is an operation which the unskilled should let alone.

Dr. Squier: I do not believe that you should condemn the operation with quite such a sweeping statement.

Dr. Cabot: I am not condemning it; I am doing it.

Dr. Squier: All right, but I doubt very much if giving a patient ether or mental suggestion or high frequency current or any such proposition would reduce a multiple arthritis in twenty-four hours.

Neither do I believe it will bring down an inflamed joint that is twice its size down to normal size in twenty-four hours. I would consider it a miracle if it did it in one case, but I have seen it with my own eyes, because I have been just as skeptical as anybody here, and I scoffed at it until I thought I had better get busy. I have had at least fifteen cases of gonorrhoeal rheumatism—whether the effect is going to be permanent or not, I am not prepared to argue—but I have seen it, it is not simply the mental suggestion; it is because that man is not absorbing infection into his blood for a few months after operation, at least.

Dr. Arthur L. Chute, of Boston: Do I understand that this operative treatment is advised in these arthritic cases only after massage has proved a failure? It seems to me that most of these people do very well under massage. As I have seen these cases, they seem to get well in time though recovery is often slow; it is only the exceptional case that does not get well after massage. For what kind of case do you advise the operation? Because you cannot feel that all these infected cases need operation? It seems to me a large operation to advise routinely in a condition where we know many get well without it.

Dr. Squier: I answered Dr. Chute in Boston, and I also made a very definite, sweeping statement out in Cleveland, that I did not think any man ought to undertake any operative procedure on the seminal vesicles until every known method of conservative treatment has been carried out. I have made that statement in Boston and in Cleveland, and in writing three times.

Dr. George S. Whiteside, of Portland, Ore.: I remember several years ago when our friend Dr. Samuel Alexander was doing prostatectomy for prostatic abscess in gonorrhœal cases, he said that after operation the men said they had what they termed "dry emissions," and in those cases where we removed the vesicles and in some cases of operations on the prostate for tuberculosis, they have had the same experience. I have not had any cases of impotence.

Dr. Hugh Cabot, of Boston: I think I ought to answer Dr. Chute's question at this point. My work has been confined entirely to the frightfully crippled cases which come to us from the orthopedic department. I do not think that even the critical Dr. Chute would deny that these patients are entitled to any benefit that anybody can give them. They are totally incapacitated. Many of them have been so for many years. They have all of them been treated as efficiently as

we know how to treat them by other methods, and I do not hesitate at all in offering them operative relief if operation will give them relief.

In reply to Dr. Squier, the effects of various accidents upon chronic arthritis are not transient. There are many cases, carefully observed by the orthopedic department, in which the effect of these accidents, such as patients etherized for some trivial purpose, has been extremely beneficial. It is a matter of common knowledge to many of the internists of the country that intercurrent disease will extraordinarily benefit these rheumatic cases, and it is quite as miraculous as any results which I have seen or heard of following operation. We must take this into consideration before we draw any startling conclusions as to the effects of operation.

Dr. Barney: In regard to what Dr. Squier has said about the possibilities of draining these organs thoroughly, I think I have seen at least two cases at autopsy where it would be impossible to feel or see these diverticula such as I have shown on the screen, because they were so buried in scar tissue that it was impossible to distinguish their presence. Therefore they will be entirely missed and not drained.

I have seen no case such as Dr. Hagner has spoken of. I cannot explain why it should occur.

In regard to the relapses in certain cases which have been treated by vesiculotomy or vesiculectomy, it is possible that there was some other focus and that the vesicle was not the sole site of infection as we thought it to be. That is a possible explanation.

What Dr. Swinburne said reminded me of a case which I have recently seen in the clinic, of a man who had had his vesicles drained by one of the members of this Society last July. He presented a definite recurrence of symptoms. His joints were considerably involved.

I have tried vasostomy in several cases, after Belfield's method, and irrigated the vesicles with argyol. I have found in quite a large number of cases that it produced a temporary benefit, but within a month or two the symptoms have returned. Also I found that vasostomy as a continuous performance is not at all feasible. In one case I irrigated the vesicles three times at intervals of a week, and each time had to dissect out the vas. On each occasion it was a little bit harder, owing to the formation of scar tissue.

None of the cases, so far as I know, in which we have excised the vesicles have complained of impotence.

NOTE: Oct. 6, 1914, a private case operated upon in June since this discussion is still impotent. There is no improvement in the joint condition. Also I have since found out that one of the hospital cases has been impotent since the operation. (J. D. B.)

I think it is fair to give all sides of the question. In two of my own cases I did not succeed in removing the vesicles. In one I got out one organ but not the other. In the second case I could not remove either vesicle. I was very much discouraged. The patients did as they have all done after operation. They exacerbated and flared up and had quite a time. Those operations were both done nearly a year ago. I have seen both patients recently, and both are cured so far as I can see.

I think that the operation should be done only after a long course of massage has proved to be useless, and, in certain cases, even if it has proved to be of value, simply because the patient has a perpetual axe hanging over him—as soon as he stops treatment the symptoms will recur. I have seen several instances where a patient has been presumably cured only to come back six months later with the same symptoms, so that he is perpetually handicapped by his disease.

THE RENAL LESIONS EXCITED BY DISTENTION OF THE KIDNEY PELVIS

By Edward L. Keyes, Jr., M. D., and Herbert Mohan, M. D.,
from the Laboratories of Bellevue Hospital

AT the present day we are all familiar with the infiltration lesions of the renal parenchyma due to pyelography. Very few such cases have been reported in detail; yet it is doubtless no exaggeration to say that every one here present has seen at least one specimen illustrating this accident, while many of us have removed several such.

The pathological picture presented by such a kidney may be briefly described as follows: The surface of the organ usually shows black patches of collargol infiltration. These patches are slightly elevated above the surrounding tissue. They consist of a swollen parenchyma, irregularly mottled with black infiltrate. The capsule, when stripped back, shows a replica of this staining.

On section we find that these patches, seen on the surface of the kidney, correspond to an underlying infiltrated area of parenchyma. Such an area is a black mottling or striation of the parenchyma, with intervening uninfiltrated spots. It may be irregular in outline but often it is definitely cone-shaped, with apex at the pelvis and base at the capsule. (In some specimens the collargol nowhere reaches the capsule.)

The pelvis itself is more or less stained and infiltrated. The cellular tissue about the pelvis may be stained black.

Microscopic examination of the parenchyma of such a kidney seems to show that the more normal portions of the parenchyma are especially predisposed to infiltration. Areas where the renal parenchyma has been completely replaced by sclerosis probably do not become infiltrated.

The collargol is found within the tubules and usually within the glomeruli as well. Moreover masses of collargol, small or large, are usually found scattered between the tubules. The kidney tissue about such masses is likely to be inflamed or necrotic.

In two fatal cases of Roessle[1] and von Hofmann[2] death was apparently due, in the one to sepsis, in the other to rupture of the kidney pelvis, with perirenal extravasation of the collargol and urine.

Such are the observed facts. Two explanations have been advanced to cover them.

Blum, Oehlecker and Tennant,[3] experimenting with cadaver kidneys, or kidneys removed from animals, independently reached the same conclusions; viz., that the collargol enters the tubules of the kidney and either passes up these to the glomeruli or breaks from a tubule into the interstitial tissue and so passes along the lymph spaces to the capsule.

Strassmann, on the other hand, experimented with living rabbits. He exposed the kidney and ureter, and made his injections very gently, so as not to distend the pelvis unduly. The collargol was retained for varying lengths of time (8 minutes to 24 hours). His experiments did not produce infarcts, left no collargol in the kidney tubules, and showed that the collargol reached the kidney capsule via the blood vessels and lymphatics and the connective tissue around the urinary canals. He therefore denies rupture of the tubules or even distension of them.

Strassmann's conclusions have been criticized by both Mason and von Hofmann on the score of neither fulfilling the clinical condition (distension of the renal pelvis to the point of colic) nor exhibiting the pathologic lesions resulting therefrom (distension of the tubules).

In order to test the validity of Strassmann's claims we have made the following experiments upon dogs:

Experiment 1: The upper part of the ureter and the kidney were exposed through an abdominal incision. One

c. c. of 10 per cent collargol solution was then injected into the pelvis. This amount of fluid seemed to distend the pelvis completely, without putting it under tension. The needle was immediately withdrawn and the kidney removed.

The kidney was hardened and stained in the usual manner. Externally it did not appear congested and no collargol was seen under the capsule or in the cellular tissue about the pelvis.

On section the pelvis was seen to be lightly stained with collargol and a few minute black spots could be seen in the cortex.

Microscopically the pelvis showed a coating of collargol. The parenchyma was almost entirely normal, but traces of collargol were found, apparently in lymph spaces, near the periphery of the cortex. There was no trace of collargol found either in the tubules or in the glomeruli or in the blood vessels of the pelvis or parenchyma.

Experiment 2: The opposite kidney of the same dog was simultaneously exposed and injected in the same manner, with the exception that the pelvis was kept distended for 15 minutes. The kidney was then removed and the dog killed before he recovered from the anæsthetic.

The gross appearance of this kidney was the same as that of the first experiment with the exception that the pelvis was more deeply stained and the parenchyma showed many black spots, while the veins outside of the kidney were somewhat distended.

Microscopically the pelvis was coated with collargol, which was also found in considerable quantity in the blood vessels adjacent to the pelvis, and to a less degree in the vessels and lymph spaces within the parenchyma. There were distinct but very slight traces of collargol in the glomeruli, either in the epithelia or in the lumina of the tubules.

Experiment 3: The dog used for this and the following experiment was considerably larger than the one previously employed. Accordingly 2 c. c. were injected to produce an

approximately corresponding degree of distension of the pelvis. A 2 per cent solution of collargol was employed. The pressure was immediately released and the kidney removed 24 hours later.

The kidney was markedly congested and there was hæmorrhage into the cellular tissue about the pelvis, within the hilum; but no trace of collargol was found on inspection, either in the pelvis or parenchyma.

The microscope showed acute congestion of the kidney, with considerable dilatation of the vessels, and parenchymatous hæmorrhages; but no collargol in the pelvis, the vessels or the glomeruli, and only a very few faint traces of collargol in the tubules.

Experiment 4: The opposite kidney of the same dog was injected in like manner, but the distension was maintained for 15 minutes. This kidney was removed with its fellow 24 hours later, and the dog killed.

The gross external appearance was similiar to that of the preceding experiment with the exception that the veins running over the kidney were more congested, the kidney rather browner, and not quite so swollen.

Section revealed a black staining of the surface of the pelvis and black infiltration of the cellular tissue about the pelvis. Many small black spots were also visible throughout the parenchyma.

Microscopically the kidney showed a congestion somewhat less intense than that found in Experiment 3 and a collargol distribution similar to but more marked than that of Experiment 2.

The collargol in the parenchyma, while still showing marked affiliation for the blood vessels, is also seen (and in rather greater quantity than in Experiment 2) in the glomeruli; and distinctly both in the epithelia and in the lumina of the tubules.

From these experiments, few though they be, we derive the following conclusions:

1. Momentary distention of the normal kidney pelvis doubtless causes no more damage than a congestion of the organ (Experiments 1 and 3), which congestion is doubtless of brief duration.

2. But, if the pressure is kept up, there is, as Strassmann suggested, an absorption of the injected fluid into the blood vessels and lymph spaces about the kidney pelvis and hilum.

3. Although, like Strassmann, we have been unable to detected any collargol forced into the renal tubules;

4. Nevertheless, we have found collargol in the glomeruli and in the convoluted tubules.

5. Yet inasmuch as there was much less collargol within the glomeruli and tubules than in the lymph spaces and vessels (and indeed much less collargol in the tubules than in the glomeruli),

6. We conclude that the appearance of collargol within the glomeruli and tubules is doubtless an excretory phenomenon.

Secondary or Late Infiltration

Such are the immediate results of distension of the normal kidney pelvis.

But a case reported by von Hofmann introduces a new question. For in this case death resulted from rupture of the pelvis, although at the time of collargol injection the patient had not the least pain in the kidney. The pain came on for the first time twenty minutes after the examination.

The history of this case is, briefly, as follows: The patient was fifteen years of age, and had had digestive symptoms for five years and colics in the left loin for a year and a half. Infected intermittent hydronephrosis was diagnosed. There was an intermittent tumor in the left loin. Ureter catheterization revealed a marked delay in indigo-carmine excretion from the left kidney and pus from this side. The statement in reference to the collargol injection

is not quite convincing. In the patient's history it is simply stated that a five per cent solution was permitted to flow slowly through the ureter catheter until it regurgitated into the bladder. The catheter was left in place for ten minutes after injection, during which time a considerable amount of collargol issued from it, and the patient felt no pain in his side, but complained of a good deal of bladder pain. Half an hour later he had severe left renal colic and vomiting. Four days later he died, the pains having continued and grown more severe, the pulse being rapid, the tongue dry, the temperature low. Post-mortem examination revealed hydronephrosis of the left kidney, pyelonephritis, abscesses in the parenchyma, infiltration of the parenchyma with collargol, and a ruptured pelvis which had caused death. In another place, however, von Hofmann states that the amount of collargol injected was 120 c. c.; its strength 4 per cent instead of 5 per cent, and that it was injected by gravity from a height of about 30 cm. He further states that the pain came on 20 minutes after the injection, instead of half an hour.

In spite of these discrepancies it seems certain that the patient did not have any pain in the kidney at the time of the injection, that the infiltration of the kidney parenchyma as well as the death of the patient were due to obstruction of the ureter from the trauma of catheterization, and that the excretion of urine into the closed sac was the cause of pain, infiltration, rupture and death.

Permit me now to report a personal observation:

The patient, an Italian laborer, 17 years of age, entered St. Vincent's Hospital in October, 1913. Apart from the common diseases of childhood, he gave no history of any significance, excepting that of his present malady. This consisted in attacks of severe pain in the left loin, radiating downward toward the groin. These pains first began in 1910, following a severe lifting strain. They varied in frequency, duration and severity; but were usually very in-

tense, lasting about two days and occurring about six times a year. He had never noticed any disturbance of urination or the passage of stone or blood in the urine. The attacks were not accompanied by any recognized fever, but were associated with nausea. He entered the hospital with no definite diagnosis, his physician even asserting that the left kidney could not be felt. But careful examination of the left loin revealed an obscure general thickening of the loin without any definite outline, tenderness, or muscular rigidity. This was assumed to be a large, low, hydronephrotic sac. Urinalysis revealed an adequate output of urea, a few pus cells, a trace of albumin and clumps of bacilli that were acid fast when decolorized for a brief period.

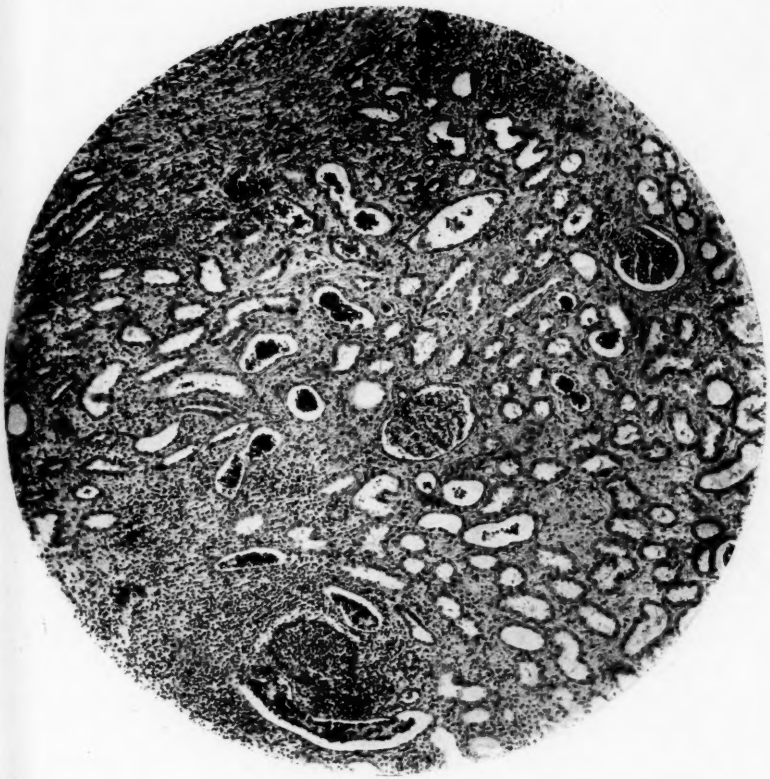
Oct. 27, 1913. Cystoscopy showed a normal bladder and normal ureter orifices. Ureter catheters passed readily to the kidney pelvis on the right side, but only ten cm. up the left ureter. From this left side a gush of some 20 or 30 c. c. of urine was immediately obtained, as though from a dilated ureter. The urine obtained from the right side showed 2.8 per cent urea and no pus. That from the left side showed 1.2 per cent urea and a few pus cells. Phenolsulphonephthalein came down strongly from the right side and very faintly from the left, but the precise data of this have unfortunately been lost.

Fifteen c. c. of 25 per cent cargentos were injected into the left ureter. Radiographs were then taken; that of the loin (though a good one) failing to show the kidney or any trace of the injected fluid; that of the bony pelvis showing the left ureter filled with cargentos up to the pelvic brim and enormously dilated.

The patient complained of no unusual pain from the cystoscopy and felt *no pain in the left loin either during the injection or subsequent to it.*

In the two days that intervened between the injection of the left ureter and the removal of the kidney the temperature rose to 101° and the left kidney became distinctly distended

PLATE IX.—Presented by Dr. E. L. Keyes, Jr.



Infiltration of kidney with cargentos. Showing areas of necrosis with black infiltration of cargentos. Also cargentos in many glomeruli and a few tubules.

and notably sensitive; but still the patient had no pain whatever in the left side.

Oct. 29. Although there was still some doubt as to the presence of real tubercle bacilli, it was evident that the kidney would have to be removed, whether tuberculous or not, since the organ itself was evidently almost entirely destroyed and the ureteral obstruction was near the lower end of the ureter. Accordinly nephrectomy was performed.

The kidney was very large and the adhesions about the vessels were so dense that it was deemed prudent to leave clamps on them. The operation was concluded in 45 minutes.

The post-operative history was uneventful. The clamps were removed on the third day. The patient left the hospital on the fifteenth day with his loin healed.

To my great surprise the kidney showed the typical lesions of injection infiltration (Plate IX). Indeed the capsule was stained with the injected fluid over an unusually large area. Section revealed the familiar gross lesions and a greatly dilated pelvis containing a brownish dilute solution of cargentos.

Microscopical section of the kidney revealed ulceration of the pelvis and extensive areas of inflammation and necrosis in the parenchyma, while the remaining tubules and glomeruli showed the familiar lesions of suppuration and dilatation. The cargentos was found distributed irregularly throughout the kidney. There were many infiltrated areas of stroma containing granular black deposit. Many glomeruli contained black granules in considerable quantity and here and there the tubules were filled with black deposit. These lesions do not differ from those that have been described in similar specimens.

This case illustrates much more clearly the point suggested by the case of von Hofmann, for in our case the patient never did have any pain. Yet the ureter was evidently completely blocked as a result of the injection, the kidney gradually filled, and the patient would have had a renal colic if opera-

tion had not been promptly performed. Yet meanwhile, without any colic and with pressure of urinary secretion only strong enough to distend the kidney, the classical infilteration took place.

This is, therefore, an example of what might be called secondary infiltration of the kidney parenchyma after injection, in contrast to the primary infiltration at the time of injection. That there is a primary infiltration at the time of the injection and caused by too forceful dilatation, we cannot doubt. It has been illustrated in many cases, notably by the case of Zachrisson. This patient received an injection of fourteen c. c. of collargol into a normal kidney pelvis and the X-ray, taken immediately thereafter, showed a shadow of collargol radiating into the parenchyma of the kidney. After five days of pain and fourteen days of fever he recovered. The opposite kidney was subsequently removed and a year thereafter the urine showed nothing abnormal excepting a few casts.

Several cases similar to this have been reported. They illustrate the fact that an immediate infiltration can take place. Yet one may suspect that secondary infiltration is perhaps more common than has been supposed. Primary infiltration is certainly painful and must doubtless result in considerable destruction of kidney tissue; but so long as focal suppuration does not intervene and the ureter remains open the patient may be expected to recover.

Secondary infiltration is obviously a far more serious accident. If the kidney is not promptly drained or removed, the ureter may remain closed and the pelvis rupture as in von Hofmann's case. Whenever an injection is made into the pelvis, in a case of renal retention, the patient must be kept under close observation and promptly operated upon, if symptoms of acute renal retention or infection supervene. Thus will the real danger of pyelography be guarded against.

On the other hand it is interesting to contemplate how the injected fluid enters the parenchyma in these cases of second-

ary infiltration. In the first place why does secondary infiltration occur at all? If the injected collargol may be forced right through the kidney parenchyma up to the capsule under no other force than that of the urine secretion itself, we must conclude that a similar condition of infiltration of the kidney parenchyma with urine must occur in many, if not in all, cases of severe renal colic and hydronephrosis. Does such an infiltration occur? And, if so, is it an element in the causation of the sclerosis that results from repeated attacks of renal retention? Furthermore it is interesting to enquire whether the pathological picture presented by a kidney subjected only to primary renal infiltration differs from that of a kidney subjected only to secondary renal infiltration?

Although we cannot pretend to answer these questions with any degree of thoroughness we have performed one experiment that throws some light upon the subject.

Experiment 5: The whole length of the ureter of a dog was exposed through an abdominal incision and three milligrams of two per cent collargol was injected into the lower end of the ureter. The black fluid could be plainly seen ascending the ureter. As soon as it reached the pelvis, and before any dilatation of the pelvis occurred, the injection was stopped and the ureter tightly ligated with silk just above the point of injection. The abdominal wound was then closed and the kidney removed 24 hours later.

This kidney showed exactly the same changes as that of Experiment 4, with the exception that grossly it showed a considerable amount of collargol beneath the capsule, (which none of the other kidneys showed). On the other hand, microscopically this kidney showed rather less collargol in the glomeruli than did kidney No. 4.

The object of this experiment was to simulate, as closely as possible, a secondary retention. The kidney pelvis was certainly not in the least distended at the time of the injection, but the collargol was left in the ureter, so as to show

the effect of the kidney secretion in producing auto-infiltration. The fact that the pathologic changes produced were quite the same as those produced by a primary retention, with the exception that the infiltration beneath the kidney capsule showed a more extensive lymphatic and vascular absorption of the injected fluid, suggests that in normal kidneys, at least, secondary as well as primary collargol infiltration is a vascular and lymphatic process, and that the collargol seen in the glomeruli and tubules is simply being excreted.

Of course, the problem in a pathologic kidney is a very different one. Yet nothing that we have seen in our specimens proves that even in pathologic kidneys the collargol goes straight up the tubules. We are now continuing experiments calculated, we hope, to determine this point.

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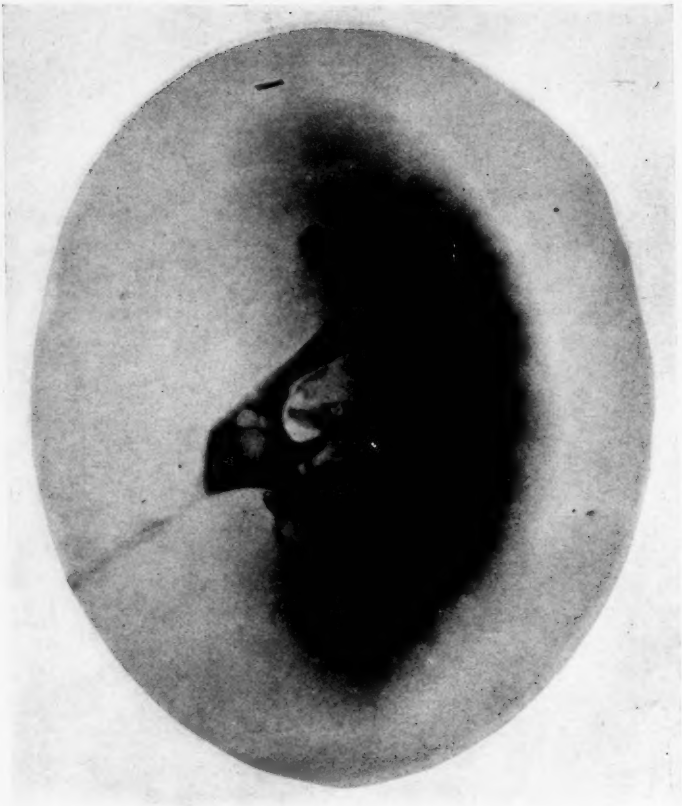
[3]Mason, Jour. A. M. A., Mar. 14, 1914, LXII, 839, has brought the bibliography up-to-date.

PLATE X.—Presented by Dr. Hugh Cabot



DISCUSSION

Dr. Hugh Cabot, of Boston: My assistant, Dr. E. L. Young, Jr., has made observations along the line suggested by Dr. Keyes and for the purpose of determining what became of collargol injected into the renal pelvis made several injections on kidneys removed at autopsy. These injections were made without the application of any considerable amount of force, the intention being to reproduce the conditions of clinical injection with a syringe.

The accompanying illustration (Plate X) shows the collargol penetrating the renal parenchyma and forming loops which closely follow the distribution of the vessels. Since it is to my mind practically impossible that the force used should have been sufficient to force the fluid into the blood vessels, it seems probable that these represent the course of the lymphatics communicating with the renal pelvis.

Dr. B. S. Barringer, of New York: I would like to ask Dr. Cabot how much collargol he has been using.

Dr. Cabot: We have been trying and intending, and I think succeeding in using an amount of fluid less than the supposed normal capacity of the renal pelvis. We try not to fill the pelvis.

Dr. Barringer: What is your gauge?

Dr. Cabot: Our gauge is the distension necessary to produce pain with boric acid solution.

Dr. Barringer: The reason I ask, it seems to me that the syringe distension will probably not exert any harm if you do not use, say, more than five c.c.; in other words, not to the point of pain, keep rather below that. While the picture obtained may not be very beautiful, it is good enough in most cases to make the diagnosis. I think that in one of these abroad cases which ruptured, they used 150 c.c. This is dangerous as well as expensive.

Dr. John R. Caulk, of St. Louis: In reference to what Dr. Keyes said and to Dr. Cabot's picture, immediately after the removal of a large hypernephroma involving the lower pole of the kidney I injected collargol and took an X-ray, which showed the same ramification clear to the cortex as the President's picture showed. We made a microscopic study and found an intertubular, —— lymphatic process.

Dr. Francis R. Hagner, of Washington: I have always used a syringe for these cases myself, and followed practically the same method

that Dr. Cabot uses—the minute the patient has any pain to stop injecting the fluid, and have had no untoward results. But lately have been using the gravity method, and believe it is safer.

There is one point Dr. Keyes brought out that I think can well be repeated: Surgeons are cystoscoping these patients in their offices, X-raying them, and letting them go home. I feel that all these cases ought to be kept in the hospital one or two days after the injection.

We had a rather interesting case in Washington this year. Dr. G. Brown Miller, a very careful man, injected a lady with twenty-five per cent argyrol. She was sixty years of age, and was supposed to have a tumor of the kidney. She had no pain at the time of the injection, but almost immediately she complained of terrible pain in the region of her heart. They discontinued, took the X-ray, and put her to bed. I saw her the next day. She had almost complete suppression of urine and some nausea. I saw her again two days later, when she was apparently in a dying condition. Her stomach was distended. A tube passed found it filled with blood. The skin was mottled with petechial hæmorrhages. She passed blood from the vagina and from the rectum. I suspected that it might be bichloride poisoning, but this was excluded. She died four days after the injection. Whether the injection had anything to do with it or not, I do not know. We were unfortunate not to get an autopsy on this case. I simply cite the case, and would like the gentlemen to bear it in mind.

Dr. Louis E. Schmidt, of Chicago: In some experimental work and in some cases where death followed the injection of cargentos, this salt has been demonstrated in different parts of the body, as the liver and intestinal tract. I do not exactly see how such large quantities can gain entrance, but evidently the demonstration of these specimens has proven the fact, for some of the patients died almost immediately following the injection.

I would like to say a word in regard to the experimental work that has been done on dogs and rabbits. None of these that I know of have been done on diseased animals, and I think that makes some difference as far as certain conclusions can be drawn. There is no doubt that nowadays we do not inject cargentos or any of those preparations into a healthy pelvis. I think that these deaths have shown that extreme care should be used. Deaths have been produced in diseased individuals, and, as I say, when it comes to making comparisons and drawing conclusions, these things must be taken into consideration.

Dr. Horace Binney, of Boston: I do not quite understand from Dr. Keyes' description just what this process was, whether it was a local process in the injected kidney, or whether the collargol entered the circulation and was excreted from the general circulation through the kidney. If so, I would expect that the other kidney would also show it. It has interested me a good deal, in the light of experimental work on dogs, described in an article by Dr. Eisendrath—I think it is in the last *Journal* of the A. M. A.—in which he injected collargol under a good deal of pressure, and he found that collargol entered the general circulation. He shows pictures of the liver and lung containing collargol. Those cases were for the most part fatal, I think, within forty-eight hours.

Dr. A. A. Uhle, of Philadelphia: The last picture projected upon the screen unquestionably shows a distention of the renal pelvis. To me it seems impossible to accurately estimate the capacity of the renal pelvis, when injected by the syringe method.

In my early work, when I depended upon the syringe method of injection, many patients complained of pain at the time of injection, which persisted for several hours. I then resorted to the method of filling the renal pelvis by means of gravity, using an ordinary graduated burette, with a short rubber tubing, to which was attached a needle with a very small opening; this does not permit of the rapid filling of the pelvis. When the level of the fluid in the burette becomes stationary, the Roentgnologist makes the exposure, and the fluid is allowed to escape.

I think there is no question that too much pyelography is being done. While pyelography is a very valuable method in selected cases, it is essential to diagnosis in but few, and its indiscriminate use should be warned against. For instance, I think just such a case as Dr. Keyes has cited, in which the spurt of urine from one catheter was normal, and a constant flow of clear urine from the other, shows that we are dealing with a hydronephrosis. I see no indication for injecting such a renal pelvis. It has been my experience that the capacity of a normal pelvis is about six to ten c. c. If more fluid is injected than the normal capacity, especially by the syringe method, even though it be a small quantity, the patient will experience pain, and such an injection may be the cause of serious trouble.

Dr. R. F. O'Neil, of Boston: I entirely agree with Dr. Hagner that this proceeding should be done in a hospital.

In regard to the gravity method, I have employed it a few times, and

while I consider it is much easier, I do not think it is free from danger. I have had patients who complained of pain, not at the time, but after they were put back to bed—quite severe colic all up and down the ureter, which was not due to over-distension, because there were only a few c. c. used. It is just as Dr. Uhle has said; this method is being used in many cases where it is not indicated.

Dr. J. D. Barney, of Boston: I have not heard mention of a method which we have used in estimating the capacity of the renal pelvis. During ureteral catheterization, methylene blue solution is injected through the catheter into the pelvis until the blue solution is seen to run out of the ureteral opening around the catheter, in this way estimate that the renal pelvis was full enough to distend the ureter and cause the solution to flow out around the catheter. In no case in which the solution has leaked around the catheter has there been pain, so far as I can remember. At any rate, I do not think it is possible for us to estimate the exact capacity of the renal pelvis, but it seems to me that this is one of the least harmful methods.

Dr. John R. Caulk, of St. Louis: Concerning what Dr. Barney has just said relative to determining the capacity of the pelvis, I have on several occasions seen the collargol flow back around the catheter before the pelvis was filled, as shown by the X-ray picture. I think we have had three cases in which we could not get a picture. We immediately got this back flow, with the catheter perfectly in place, but we were unable to get anything to stay in the renal pelvis.

Dr. Cabot: You would not get over-distension.

Dr. Caulk: No, but you could not tell the capacity of the pelvis.

Dr. Cabot: What Dr. Barney is getting at is that it is a safe method.

Dr. Hugh Cabot, of Boston: I had taken from our records two cases which bear on the point raised by Dr. Hagner that the doing of these injections in one's office, or as we have done them in the past as occasional procedures in the out-patient clinic, is not a good plan. One patient of twenty-nine was cystoscoped by Dr. Barney. He came in with symptoms of left renal colic. He had a practically normal urine. He was negative to X-ray for stone. The left kidney could be indistinctly felt. The pelvic capacity was estimated by the manner that Dr. Barney has just described, and after putting in fifteen c. c. of indigo-carmine solution there was no extra catheter flow, no pain or discomfort. An injected radiograph was then taken, which showed a large left renal pelvis. He returned to the out-patient department, was pretty uncomfortable, and

we decided to keep him in the accident room, which is our overflow for that purpose. He had a lot of pain during the next two days. On the third day he was operated upon, not on account of his pain, but on account of the evident enlargement of the renal pelvis. The kidney was substantially similar to that just described by Dr. Keyes, the dark staining of the cortex, the staining of the pelvis, and brownish, œdematous infiltration of the peripelvic fat. It did not appear that any permanent damage had been done. The capsule was stripped, the kidney suspended, and it apparently behaved perfectly well, but we did more damage than we intended to.

Another case occurring within a month of the first one was that of a man of twenty-seven, with right side pain, who came to us for a differential diagnosis between some renal lesion and an appendix. An X-ray for the purpose of excluding stone showed a large right kidney but no stone. There was extra-catheter flow, without pain, at distension of twelve c. c. Then with a little more distension he had some pain, which was like the pain that he had had before, from his so-called appendix. In other words, we reproduced the pain. We introduced ten c. c. of collargol, took a picture and sent him home. He came back next day complaining that he felt sick and looking the part. We sent him into the hospital. He had a slight rise of temperature, and persistent pain, and three days later was operated upon because of his pain—a case in which we believed we had done a sufficient amount of damage to the kidney to require us to look into it. He had a black stained kidney cortex, a not much enlarged kidney, but distinctly irregular on the surface. The operation was done by Dr. Smith. There was a great difference in the apparent density of the kidney, and widespread collargol infiltration with œdema. The capsule was stripped, and he did perfectly well. The urinary examination at the time of his discharge, fourteen days later, showed normal urine.

I think these cases bear upon the point raised by Dr. Hagner. This was a year ago. We do not do that now.

Dr. Keyes: In reference to the specimen shown by Dr. Cabot, in the first place, it seems to me the picture there of the obviously looping, vascular or lymphatic distribution of that injected fluid suggests the point that has already been brought up by Straussmann; that perhaps post-mortem injections do not precisely simulate the pathological findings in living specimens, for certainly such a specimen as that has not been shown on any living specimen.

I too have read Dr. Eisendrath's observations. It is quite apparent that he put his collargol in with a horse syringe. That, of course, is not to be taken as clinical evidence, and he stated in his article that his report was only a preliminary one. We have no right to criticise his observations. They do not, so far as published, have any bearing on clinical disease.

Two of my experiments here bear upon Dr. Binney's question as to whether the excretion is local or general. The same dog had experiments three and four performed on him. Obviously if there had been a distribution of the collargol from either of these injections and a re-excretion, the re-excretion would have taken place as generally in kidney No. 3 as in No. 4. As a matter of fact, you see none in No. 3, and microscopically you find it only with great difficulty, whereas you easily see it in No. 4.

The remarks on conservatism made by so many of the gentlemen I heartily concur in. I find it necessary to inject collargol very, very rarely nowadays, and I keep the patients in the hospital and inject by gravity.

Colic was mentioned by Dr. O'Neil as coming on some time after injection. Such a colic as that may come on after any ureteral catheterization. If the kidney is reasonably normal, even if we do infiltrate the pelvis we do not cause any permanent damage, unless there is enough infection to set up an acute suppuration, or if as in the case mentioned by Dr. Hagner the patient is ill enough to make it dangerous to add anything to the condition that already exists. But if there is any retention the injection may cause grave secondary phenomena.

Finally, I do not see why distension of the pelvis with methylene blue is any better than distension with other kinds of black.

Dr. Cabot: That is just to have some color come out around the catheter.

Dr. Keyes: That ought to do just as much harm. Your friend Dr. Draper Marrey showed that he got nephritis from injecting water into the kidney pelvis.

OBSERVATIONS ON THE GROWTH OF THE GONOCOCCUS AND THE STAPHYLOCOCCUS ALBUS FROM THE URETHRA IN PLATE CULTURE. (A CRITICAL STUDY OF WARDEN'S PUBLICATIONS)

By E. G. Crabtree, M. D., of Boston
(By invitation)

THE outlines of bacteriology are becoming indistinct in the middle west, where the identity of well known bacteria has recently received considerable attention and some questioning at the hands of progressive bacteriologists. Variations in the so-called "characteristic properties," which serve as criteria in the differentiation of bacteria, suggests the possibility that our predecessors have confused bacteriological terminology by assigning names to many variations of a few bacteria when growing in widely dissimilar environment. Much valuable knowledge will undoubtedly be obtained by a review of the subject of bacteriology in the new light, yet this must needs be done with care. The Gram stain, a variable quantity in itself, and the elusive phantom—morphology—can not give conclusive evidence that two heretofore considered different bacteria are the same, as has been contended in Warden's publications of last year which we are about to discuss.

Warden,[1] in his studies of the gonococcus and the staphylococcus albus from the urethra, concludes that "Many, if not all, of the Gram-negative, intracellular, biscuit- or coffee bean-shaped cocci, observed in the purulent discharge in acute gonorrhœa, which are regarded as gonococci, and which serve as criteria of diagnosis, are not gonococci, but belong

to the staphylococcus group. The provisional name staphylococcus urethræ is suggested for this coccus.

As a basis for this conclusion, Warden cites his observations on the Gram staining properties of the staphylococcus albus, obtained from urethral pus, (1) when grown on the usual media in pure culture and in mixed culture with the gonococcus, (2) when within leucocytes, in phagocytosis experiments made to obtain intracellular organisms, and (3) when in colonies.

Pure culture of the staphylococcus albus and gonococcus were grown on plate culture media spread in thin films on microscopic slides which permitted staining and study of colonies in situ.

Further peculiarities in Gram staining, together with changes in morphology, of the same staphylococcus, were observed when the organisms were in suspension in body fluids such as blood serum, hydrocele fluid, and ascitic fluid; and in ferments, such as dilute pancreatin, leucocytic extract, and gonococcus autolysate. These fluids are of significance in the study because serum, the product of dead leucocytes or leucocytic extract, and the by-product of gonococcus growth, gonococcus autolysate, are present within the urethra in urethritis.

Briefly Warden's observations are as follows:

(1) In suspensions of staphylococci, the Gram staining properties of the cocci vary according to the nature of the fluid from which the suspension is made. Cover-glass preparations made from this coccus in normal salt solution, show the familiar Gram-positive staphylococci, even in suspensions which have stood at room temperature for weeks. But 3 to 6 hour old suspensions in serum, hydrocele fluid, ascitic fluid, weak pancreatin solution and gonococcus autolysate show Gram-negative forms of the still viable cocci.

(2) The intracellular diplococci in gonorrhœal pus are probably Gram-negative staphylococci in diplococcal form.

Phagocytosis experiments demonstrate both the ease with which staphylococci, especially when in the Gram-negative state, are taken up by leucocytes, and the difficulty experienced in ingesting gonococci. Granting that the same conditions exist in the urethra as in the laboratory, the gonococcus is not an organism to be expected within the cells in gonorrhœal pus.

(3) The staphylococcus grows, for the first few hours, as a Gram-negative organism. When staphylococci are planted on plate slides and stained at various stages of incubation, small, young colonies, up to 8 to 12 hours of age, show all Gram-negative cocci. Older colonies show Gram-negative bases, where the cocci are still in contact with the culture media, but in those portions of the colony which are elevated above the surface of the media the cocci are Gram-positive.

Before proceeding to a discussion of these findings, I wish to mention some general considerations pertinent to the cultivation of the gonococcus and staphylococcus albus, some of which have been discussed in Warden's publications.

Staphylococcus albus and gonococcus colonies, when grown on the usual media, are unmistakably different. Macroscopically the large, slightly elevated, whitish colonies, which are seen at the end of eighteen to twenty-four hours incubation, are in no way to be confused with the smaller, transparent, dew-drop colonies of the gonococcus. Colonies of gonococci more than twenty-four hours old become more opaque but never white to such an extent that one would confuse the growth with that of the staphylococcus on the same media. Microscopically the two types of colonies are distinguishable. These small colonies are best compared by planting mixed suspensions of gonococcus and staphylococcus on slide plate media, such as Warden uses, and staining in situ. Eight to twelve hour growths are best for the purpose, since the colonies at this time are composed of but one to three layers of bacteria. The staphylococcus

colony appears as an irregularly bunched, elevated colony of cocci and diplococci two to three layers thick at the centre to a single layer at the periphery, with coccal forms predominating. The gonococcus colony is smaller, flat, and composed chiefly of diplococci arranged mosaically over the surface of the media, in a single layer. Both types of colonies stain Gram-negative at this stage of incubation but in spite of the similarity, are easily distinguishable by the arrangement of the cocci composing them.

Pure cultures of the gonococcus are seldom obtained from pus in gonorrhœal urethritis. Pus from early cases of urethritis, from two to six days after the appearance of the discharge, usually shows a preponderance of gonococcus colonies and a much smaller number of staphylococcus colonies. In the late cases, two or more weeks from the appearance of the discharge, the growth is chiefly staphylococcus with a few gonococcus colonies.

Diplococcal forms are common in any rapidly growing coccus. The phenomenon is due to rapid growth with incomplete mitosis at the time of observation. Perhaps, in the best cover-glass preparations, morphology may be sufficient for differentiation of these cocci from gonococci, but this is not the case where the bacteria are stained on plate culture media as in Warden's experiments, or clinically, where other bacteria and much cell detritis is present.

The Gram method of staining bacteria is not absolutely reliable, and numerous variations in the reaction of the same bacteria in changed environment have been recorded. Eisenberg[2] has shown that Gram-positive bacteria, undergoing spontaneous degeneration, a process of bacteriolysis, become Gram-negative. Also Gram-positiveness of bacteria is influenced by serum without the picture of bacteriolysis being presented. The cocci are still viable. The causal agent is not mentioned.

Schmidt[3] working with colon bacilli found the organisms Gram-positive when occurring in fat stools. Cedarkreutz[4]

found some cocci and B. coli Gram-positive in butter. Nikitin[5] found that by treating some varieties of bacteria with fat-splitting ferments they became Gram-positive. Grimme[6] records the observation that weak alkalies, 5 per cent hydrochloric acid, pepsin and trypsin will produce Gram-negative bacteria.

Eisenberg[7] expresses the opinion derived from observation of the work of others as well as from his own study, that introduction of albuminous material into bacterial preparations and culture media produces Gram-negative bacteria in some cases.

Of those recorded observations, that which most concerns the question in hand is the effect of albuminous material on the Gram stain. Inasmuch as the gonococcus is difficult to grow in culture except on special media containing blood serum or its equivalent, albumin is present in Warden's experiments wherever Gram-negative staphylococci are seen. The same condition obtains in his suspensions of the staphylococcus in body fluids, but is absent in the normal salt suspension in which the cocci remain Gram-positive. Similarly, gonococcus autolysate contains albumin as a requisite to the growth of the gonococcus which produces it. Experimental evidence has already been indicated to show the possibility of ferments being further factors in producing Gram-negative bacteria in this case.

I have endeavored to repeat Warden's experiments exactly as described by him with the exception of two minor variations from his technique, viz.: (1) hydrocele agar was substituted for ascitic agar since our cultures grew better on this media as we prepare it in this laboratory than on our ascitic fluid agar; (2) the substitution of Bismarck brown for 1 per cent carbol fuchsin as a counterstain.

Our findings are essentially the same as those reported by Warden. Gram-negative staphylococci are readily produced by treatment of Gram-positive organisms with serum or body fluids and by the ferment action of leucocytic extract,

gonococcus autolysate, and pepsin. By phagocytosis of these Gram-negative staphylococci, leucocyte containing organisms similar to those found in gonorrhœal pus were produced. But Gram-negative staphylococci in cells were not sufficiently characteristic to deceive the house officers of this hospital and the diagnosis of gonorrhœa was not once made. Gonococci, also, can be taken up by leucocytes, in vitro, but less readily than Gram-negative staphylococci.

On plate cultures the staphylococcus grows in Gram-negative form in cultures up to eight hours of age; then the cocci become Gram-positive in all portions of the colony where the individual organism is not in contact with the surface of the media, i. e., in that part of the colony which is elevated above the surface of the media.

Further experimentation with plate cultures revealed significant facts. A suspension of the same staphylococcus was made in normal salt solution. Cover-glass preparations from this suspension showed all Gram-positive cocci. With material from this, suspension plates were prepared and stained immediately without incubation. The cocci on the plate were Gram-negative. If, however, the bacteria from another slide like the first be washed off into salt solution again, they stain Gram-positive as before. Gram-negativeness appears to depend entirely on the staphylococci being on the plate media. Staphylococcus albus and aureaus from abscess, and streptococcus obtained from the throat were also found to be Gram-negative when on the plate media, but Gram-positive when in water or salt solution suspensions.

If a small colony from a six to eight-hour slide plate culture of staphylococcus albus is removed from its bed and washed in normal salt solution and the plate stained as usual by Gram's method, and cover-glass preparations made from the suspension containing the colony removed, the colonies remaining on the plate are all Gram-negative and the cocci in suspension Gram-positive. These facts lead one to doubt that the staphylococcus normally grows Gram-negative for

a time, but to believe rather that the appearance of Gram-negativeness is due to an artefact. If so, what is the causal factor? Extracts of hydrocele agar, which contain phosphates and other salts do not produce Gram-negativeness. Thin films of celloidin when replacing agar on plate slides, cause no loss of Gram staining ability. Variations in the time allotted to each step in the performance of the stain makes no essential difference in the results except that in some cases where methyl violet has been allowed to remain in contact with the bacteria for considerable time decolorization is difficult and the bacteria show dark centers with Gram-negative peripheries.

While working with suspensions of staphylococci in hydrocele fluid which contained small flakes of albuminous material, it was observed that in cover-glass preparations all the cocci lying free in the field were Gram-positive, while those beneath the flakes were Gram-negative. Also in suspensions made in blood serum which contained a few red cells in clumps, the cocci which were entangled in cell clumps and fibrin were Gram-negative but elsewhere Gram-positive. These observations were made in newly prepared suspension where no incubation had taken place, nor had the preparations been allowed to stand for some hours—two conditions which Warden considers essential for the production of Gram-negative staphylococci in serum. Albumin clearly interferes with the staining properties of these organisms. Experimental evidence tends to show that by long contact with serum, the bacteria become sufficiently coated with albumin to respond abnormally to the Gram stain. That there is no permanent change in the bacteria themselves is demonstrated that after washing in salt solution they again become Gram-positive. Gram-negative cocci are viable since cultures from suspensions in which Gram-negative forms predominate show abundant growth. Mechanical interference by albumin clearly explains the growth of Gram-negative colonies from Gram-positive cocci. A thin layer of

albumin, derived from the media, covers the surface of the media to a sufficient depth to submerge the small colonies but apparently does not in any way interfere with growth. When the colony attains sufficient size to rise above the high water mark of albumin the top stains Gram-positive while the portion remaining below the albumin continues to stain Gram-negative.

Gram negative staphylococci can be prepared at will. If a dilute solution of egg albumin is applied to a portion of the surface of a cover-glass preparation of staphylococci with a camel's hair brush, and the Gram stain applied, it is found that in the portion of the preparation covered by albumin the cocci are Gram-negative, elsewhere they are Gram-positive.

Granting Warden's hypothesis that conditions found within the urethra in gonorrhœal urethritis and laboratory conditions are essentially the same, he produces insufficient evidence for the sweeping conclusion that "Many, if not all the intracellular, Gram-negative biscuit- or coffee-bean shaped diplococci, commonly called gonococci," are staphylococci masquerading in Gram-negative form.

He believes that Gram-negative diplococcal forms of staphylococci are indistinguishable from gonococci when seen in the ordinary cover-glass preparations from pus.

By phagocytosis experiments he shows that the Gram-negative staphylococcus is more likely to be the organism within the pus cell than the gonococcus. As a further argument he states that the staphylococcus normally has a Gram-negative stage of growth eight to twelve hours long, which stage is the one seen in acute gonorrhœa. The chronic gonorrhœas represent late stage Gram-positive growth of the same organism.

He has failed to note the fact that the Gram method of staining bacteria does not give uniform results when applied to bacteria in unusual surroundings, especially in the presence of fats and albumin. The Gram stain, although sup-

ported by the doubtful aid of morphology, does not warrant the reliance placed upon it.

He has also entirely neglected cultural evidence of the presence of the gonococcus in gonorrhœa. The large proportion of gonococcus colonies and the few staphylococcus colonies seen in cultures taken directly from urethral pus in early cases of urethritis seems to indicate beyond doubt that the intracellular diplococci "commonly called gonococci" and which produce gonococcus colonies, are not staphylococci; and that the reason so few staphylococcus colonies appear is that few are present in the pus. If the intracellular organisms are viable Gram-negative staphylococci, which laboratory experiments show to be possible, there should be a growth of almost pure staphylococcus evidence of gonococcus colonies. We have absolutely no evidence to show that staphylococci in their Gram-negative state ever produce the gonococcus type of colony, or organisms which behave in culture like the gonococcus. What staphylococci there may be present in acute urethritis pus may be in Gram-negative form; indeed there is reason to believe that they are Gram-negative in pus with the high albumin content of acute urethritis pus.

I have shown that the staphylococcus does not have a short Gram-negative stage of growth, but that this is an illusion produce by mechanical interference of albumin with the staining reaction of the bacteria, either by preventing penetration of the basic stain or interference with fixation of the stain. Plate culture media, which contains considerable albumin in added serum, hydrocele or ascitic fluid necessary to the growth of gonococci and used by Warden for the staphylococcus also, is covered over with a thin layer of albumin deep enough to cover the small colonies completely and the bases of the larger ones producing Gram-negativeness in those portions.

The morphology of cocci and diplococci, except perhaps in cover-glass preparations from pure culture, gives little aid

in differentiation and is an even less valuable aid in pus with cell detritis. I have been unable to produce "pus cells" containing Gram-negative diplococcal staphylococci which are indistinguishable from gonorrhœal pus, although the resemblance was close in some cases.

By repeating Warden's work I have been led to conclude:

(1) That Gram-negative diplococcal staphylococci may be produced in the laboratory, and it is conceivable that they also occur in the urethra, but Gram-negativeness is due to mechanical interference of albumin with the Gram stain.

(2) The staphylococcus albus has no stage of growth in which it is a Gram-negative organism. The appearance is due to an artefact.

(3) The evidence presented by Warden that gonorrhœal urethritis is due to the staphylococcus albus is based upon erroneous interpretations of his observations and is fallacious.

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DISCUSSION

Dr. Hugh Cabot, of Boston: I want to read into the record the following statement: This work was started by Dr. Warden's coming to the hospital and asking us to go over his work with a view to substantiating his conclusions or disproving them. He brought us a large amount of very beautifully prepared material apparently bearing out his conclusions. I asked Dr. Crabtree to take up the pathological side of the work, and you have heard his conclusions. I asked Dr. Smith to take up the clinical side and these are the conclusions which he has drawn. We tested the method both in the laboratory and in the clinic.

"At the time Warden presented his thesis personally to the members of the genito-urinary department of the Massachusetts General Hospital in November, 1913, he proposed a method for applying his theories to the practical treatment of gonorrhœa. He suggested the use of a sensitized vaccine made from the staphylococcus urethræ, and at the same time the use of a serum derived from goats immunized to staphylococcus urethræ and to the gonococcus. These measures he himself had used in a few cases of acute gonococcus urethritis with excellent results, getting cures within a week. He presented us with a number of bottles of the vaccine and the serum, prepared by himself, and gave definite directions as to their use. He requested that the cases on which this method be tried be early ones in which the discharge had been present for not more than one week. He advised the injection on the first day of 15 c. c. of serum and 300 millions vaccine; on the second day, 15 c. c. of serum and 600 millions vaccine; on the fourth day, 600 or 900 millions vaccine; on the sixth day, 600 or 900 millions vaccine. The patient might have sandalwood oil internally, but no silver preparations locally.

These directions we tried to follow literally. The patients were given the serum and vaccine as advised by Warden, were given sandalwood oil and told to drink quantities of water and to eat only a simple diet. No local treatment was used. Five cases of acute urethritis, all first attacks, with duration of one to eight days, were given the treatment. All had at least two serum injections and three vaccine injections, which, according to Warden, should have given results. One case did very well. At the end of one week his urines, both of which had been cloudy, became clear; he had a mucoid discharge which showed epithelial cells, no pus cells, and large quantities of large, Gram-negative, extracellular

diplococci, thought by Dr. Wright to be not gonococci. The other four cases showed no better results than one would expect from forced fluids and santal oil. Three developed posterior urethritis; one of these, a little later, a very acute epididymitis. The fourth remained about as when first seen.

One chronic case with gonococcus joints (strongly positive blood) was tried, but although three doses of serum and four of vaccine were given, no result was observed, save an increase of his urethritis.

The treatment caused no ill effects, such as malaise, chills, etc., except a very marked urticaria coming on about one week after the use of the serum, and lasting from twenty-four to forty-eight hours.

Dr. Louis E. Schmidt, of Chicago: I have been doing some of this work for the last several years with Warden, and am in a position to discuss this, as far as the clinical aspect is concerned, because he has done practically three-quarters of the work in my office. So I am in a position to state something in regard to some of the results that he has published. I am not in a position to state the exact number of cases treated, but I should say somewhere in the neighborhood of seventy-five, and they have included all classes of cases—acute cases particularly, and also some cases in connection with complications in the joints.

My personal impression is that the results have not been any different from the vast majority of cases that are treated by any other method, whether with vaccine or simply with medication and with local treatment. I do not think that a limited number of cases, however, gives us permission to absolutely condemn a method of procedure or treatment of a condition that is so unsatisfactory to treat with any method.

I wish to say one word in regard to bacteriological findings. Men like Novy of the University of Michigan and Hektoen of the University of Chicago, who is editor of the Journal of Bacteriology, both have urged him to continue in this work, with the belief that there might be such a possibility as to the correctness of his findings.

To sum up the whole substance in a few words it is this, that the staphylococcus pyogenes albus urethræ in the presence of the gonococcus changes its general characteristics so that in due course of time the gonococcus is extinguished and that the germ that we always believed to be the gonococcus in all the average stained specimens that are made from any urethral smear is not the gonococcus but the staphylococcus

albus urethræ. That is Warden's conclusion. I do not know whether he has in Boston, but in Chicago he demonstrated in such a manner the whole series of bacteriological findings so that the average man, who is more of a practitioner than a bacteriologist, is much impressed with this demonstration.

From these pictures here I was not impressed as to the complete exposure of all the different steps. Those who have seen Dr. Warden's demonstration will know that it takes quite an expert to be absolutely dissatisfied with his beautiful demonstration of the staphylococcus changing its morphological characteristics and staining properties, etc., to a diplococcus.

As far as I am concerned I wish to say that I am very skeptical about the proposition, but it is such a wonderful exposition about bacteriological conditions that I think it requires a great deal of study. Practically all bacteriologists are opposed to his conclusions, but I think he should receive as much credit as possible for trying to work out this problem, particularly when you consider all this vaccine treatment that is undertaken in a reckless sort of way. He is at least trying to bring it down to a scientific basis.

Dr. Cabot: I should like to say that this work was done extremely carefully. Dr. Warden came to us in the best of good faith, and showed us a remarkable collection of pictures. The thing was discussed with the members of the department. Dr. Wright seemed to put his finger at once upon the weakness of this situation. He said, "Nothing can be proved by stained preparations. The question is whether this staphylococcus grows like the gonococcus. The gonococcus grows one way, the staphylococcus grows another, unless we are wrong in believing that two separate organisms exist. The final proof must come from the demonstration by culture, not from staining. Almost any illusion can be produced by staining. Cultures never will look alike, and there is the key to the situation."

It was under Dr. Wright's supervision that Dr. Crabtree did this work. Personally I was much in sympathy with Warden in his conclusions. It did not occur to any of us to believe that Warden was trying to deceive anybody. His work was thoroughly and carefully done, and he has been a careful and conscientious observer. If error has crept in here it has crept in by the failure to sufficiently use cultural methods, which I believe to be the source of error. I think Dr. Crabtree, though, has

shown us the possibility of the production of artefacts in staining methods which enabled Warden to reproduce pictures showing the cocci entirely indistinguishable one from another.

Dr. Louis E. Schmidt, of Chicago: I would like to say one word in connection with that. I will defy anybody, as far as these statements are concerned, to show that he depends on any of the staining processes. He does not. Second, his bacteriological training and ability to recognize the differences in forms of the different cultures and colonies is quite satisfactory, taking into consideration his standing as a bacteriologist. And furthermore, anyone who has seen his work, I do not believe, can point out those differences that Dr. Crabtree had reference to. The illusion is absolute, so complete that I do not believe that they can be pointed out. That there may be or probably is an error I believe, but I do not believe that it has been demonstrated satisfactorily to all concerned up to the present moment.

Dr. William E. Lower, of Cleveland: It seems to me that clinically the work of Warden has been confirmed. There are certainly a number of these cases of chronic urethritis with intracellular and Gram-negative diplococci which give a complement deviation negative, and which clinically do not resemble gonorrhœa, even though organisms are present which resemble the picture-book gonococci. The cases we have observed which have had a complement deviation negative have not infected anyone. We have said these cases are not gonorrhœal and I was hoping Warden had found an explanation of them. Clinically his explanation seems satisfactory although I am not prepared to say whether or not it is true bacteriologically.

Dr. Edward L. Keyes, Jr., of New York: Let us go back to the question of chronic urethritis, about which we can talk for a day or two without reaching any conclusion, but which has been opened up by Dr. Lower. Those Gram-negative diplococci, extracellular and intracellular, not only appear in cases of chronic urethritis, but they appear in the urine of patients who never have had any gonococci. I never made the investigation, but I have no doubt that a five-year old child will be likely to show these same organisms. They are the types of staphylococci which I have been in the habit of calling degenerative forms. They may perhaps be albuminous forms, as the reader of the paper has suggested; they may not be degenerative forms, but they are types of staphylococci which under certain circumstances do become Gram-negative. They have

no reference to the fact that the patient has had an antecedent gonorrhœa. We cannot make a diagnosis of the gonococcus in a case of chronic urethritis except in two ways: in the first place, by culture, and, in the second place, by the complement fixation test. I do not think that a microscopic examination has any value whatsoever. On the contrary, I am bored to death, as I have no doubt a great many of you are, by practitioners who daily bring to me patients suffering from no symptoms whatsoever, but in whom that doctor is able to demonstrate Gram-negative diplococci. I tell them I am sorry but I am not interested. Of course, there are cases where we have difficulties. But I do not think those diplococci should worry us.

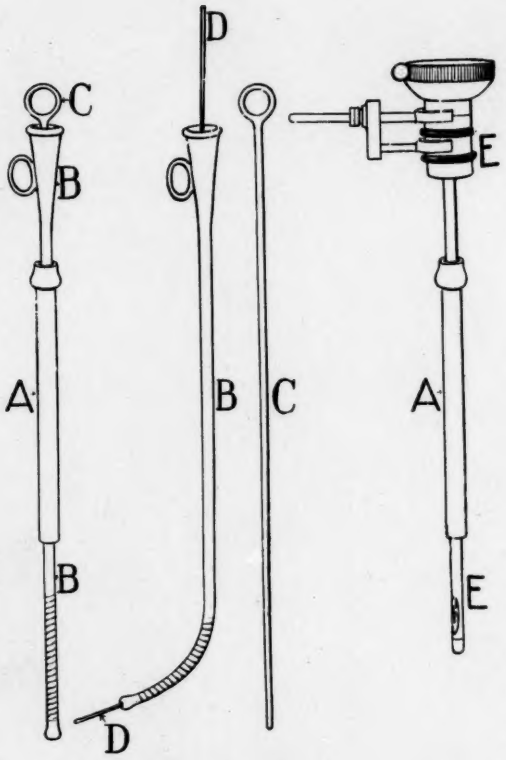
Dr. Francis R. Hagner, of Washington: I have a patient that I have been interested in for a year and a half. This man had some years ago a gonorrhœa, and he is apparently perfectly well. He has no shreds, no pus in his urine. A smear from his anterior urethra will just show myriads of organisms. These organisms most of them are Gram-positive, but every now and then you will find a few Gram-negative. That man has a negative complement fixation test, and I have had cultures taken from his urethra for the gonococcus quite a number of times. They have always been negative, and we have never found anything except staphylococci. He is perfectly well, and yet for a year he has had myriads of staphylococci in his urethra.

Dr. Schmidt: I would like to say that this whole argument proves to a certain extent Warden's contention.

Dr. Crabtree: I have seen in the course of the last three or four months in the female department of the out-patient clinic four or five cases which showed Gram-negative forms of intracellular diplococci which were apparently not gonococci. One of them was the case of a young girl who had had a discharge for two days, and the house officer brought the slide to me and said that he had made a methylene blue smear from it and it showed blue intracellular diplococci which he could not tell from the ordinary gonococcus. But on staining by Gram's method they stained almost without exception Gram-positive. Some, however, were Gram-negative. We took cultures from the vagina on hydrocele agar and the cultures grew out as Gram-positive organisms, without any Gram-negative organisms at all. Perhaps it was just fortune that we did not get any Gram-negative organisms in this case, but there were none at all that were Gram-negative in the culture.

It seems to me that the important thing that Dr. Warden's work has demonstrated is the recognition of the Gram-negative forms of Gram-positive bacteria which occur in the urethral and vaginal smears in the presence of albumin. It points out that we need to be a little more careful in our diagnosis of the gonococcus and watch for the Gram-negative forms of other organisms, which I firmly believe do exist, and are sufficiently numerous as to be confusing in some cases.

PLATE XI.—Presented by Dr. C. H. Chetwood



A NEW AND RAPID METHOD OF OPERATING UPON IMPASSABLE URETHRAL STRICTURE

By Charles H. Chetwood, M. D., LL. D., of New York

THIS technic has already been reported upon, but since then other features have been added, and continued experience in its use has served to strengthen its claims in my estimation as a method to be relied upon in a certain number of cases. All operators come in contact with a varying percentage of cases of impassable stricture, by which is meant, impassible to any instrument in the hands of the particular operator, whether or not the flow of some urine may be possible. This percentage is to a certain degree a personal equation, and therefore the adoption of this technic is also more or less governed by the same factor. Personally, I have never been compelled to perform suprapubic cystotomy for retrograde approach to the urethra, owing to an unsuccessful attempt to find the urethra by way of the perineum. But I must own to having been in some very bad situations, involving the expenditure of a laborious length of time, in attempting external urethrotomy without a guide for impassable stricture. And in certain cases this difficulty has been as much in finding the distal or anterior section of the urethra as in locating the proximal or posterior end. For the latter, the method suggested by Young, of reaching the apex of the prostate, may be used most effectively. It is not, however, from this point, in my hands, an easy matter to find the curve of the urethra anteriorly, in cases where the stricture area is exceedingly tortuous and buried in nodular tissue. The intermediate portion may be resected, and the finding of the distal or an-

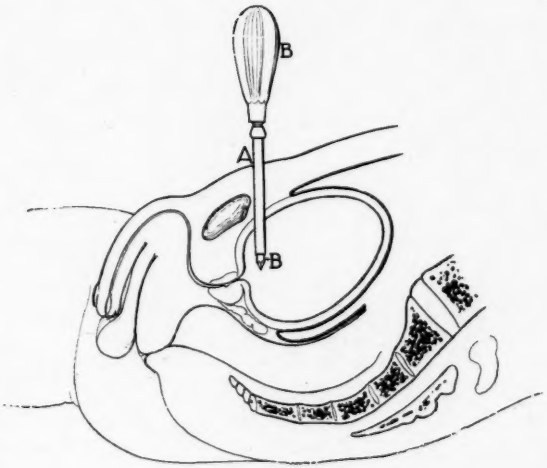
terior portion may in some instances be materially facilitated by resorting to the expedient suggested of injecting an aniline fluid through the meatus, so as to stain the urethral tract at the site of the perineal wound. But this attempt is not always effective, when pockets and false passages exist.

The technic in question was originated by my Chief of Staff, Dr. D. A. Sinclair; the application we have mutually employed. The result is a complete mastery of the difficulties of an otherwise troublesome complication.

It is desirable that the patient retain urine for several hours previous to operation. This is obviously unnecessary in cases of complete retention. The operation is performed under local or general anæsthesia, as preferred. A cannula and trochar, about 16 French calibre, is thrust just over the pubis in the median line, into the bladder, with the latter under distension, either with urine or antiseptic fluid forced through the urethra. When the trochar is removed, the fluid is emptied and the bladder is cleansed as before a cystoscopy. Now, through the cannula, is introduced an indirect lense telescope of proper calibre to fit the tube. Through this the internal orifice of the bladder is viewed with absolute clearness. The distal aperture of the cannula is then applied to the internal orifice, the telescope removed, and a curved, flexible obturator is introduced through the cannula, which readily enters the prostatic urethra, and may be advanced as far as the apex of the prostate, or even the membranous canal. A finger introduced into the rectum will determine the success of this procedure.

The flexible obturator is provided with a fine interior lumen, through which a filiform or whalebone bougie may be introduced, which often finds the proximal opening of the stricture, and then may be advanced until it appears at the meatus. In such instances the remaining steps of the operation are much simplified. A perineal cut is made on the point of the obturator, the bougie guide is drawn through the perineal opening, and a small urethrotome knife with an

PLATE XII.—Presented by Dr. C. H. Chetwood



eyelet is fed thereupon, toward the anterior section, and the strictured area is incised. In cases where it is not found possible to advance the bougie to the anterior urethra, the method of injecting an aniline blue solution is adopted, but through the canal of the obturator, while it is applied directly to the point of obstruction. In either of these instances the performance of the operation is simplified, and, as compared with the older methods of suprapubic retrograde urethrotomy or perineal section without a guide, its duration is surprisingly short.

DISCUSSION

Dr. J. Bayard Clark, of New York: I have had the pleasure of seeing Dr. Chetwood and Dr. Sinclair do one of these cases and it is a surprisingly neat operation, in which the internal urinary meatus can with such facility be seen and catheterized; so that it seems that it ought in certain cases to offer a very useful means.

Dr. C. L. Gibson, of New York: The question of operating on so-called impassable strictures has always been a very fascinating one for me, and I have been guilty of various modifications of the operation at various times. There are two points to be considered; in the first place, the question of doing a urethrotomy without a guide, and having accomplished that, the question of finding the posterior end of the urethra. I personally do not admit that there is any difficulty in doing an external urethrotomy without a guide. Any case where one has to operate for a stricture without a guide will have a membranous urethra so dilated that if one keeps in the middle line it is almost a physical impossibility to miss the urethra. Now where I think the trouble comes in is that men open the urethra when operating without a guide and fail for the lack of a proper armamentarium to demonstrate that they are in the urethra. Men will insert a catheter of small caliber that gets stopped up with a blood clot, they feel sure they are not in the bladder, and get panic stricken. Having opened the urethra and got into the bladder, I personally do not care what happens, because I have got what I am after, drainage of the bladder. I know that within a week or ten days I can slip a filiform bougie right through that tortuous urethra, which has been put in a state of rest, which has been impassable in a state of inflammation and this I have done twice.

There are very few cases which admit of any great difficulty in finding the posterior opening of the anterior urethra. If there is any difficulty, a little device which I have employed a good many times has with one exception made it perfectly easy to me. I pass my special filiform cannula* sound and put it into the upper end of the urethrotomy opening, point it up toward the urethra and jiggle through it a filiform, which nine times out of ten will walk right up through the urethra.

I think this is an awfully ingenious thing of Dr. Chetwood's, but I rather hate to see the operation of external urethrotomy looked upon as

*This is practically a Bellocq sound without a spring.

such a grave and extraordinary thing. It is looked upon as a grave and extraordinary thing because it is done so often nowadays in such a grave and extraordinary fashion. I know men nowadays do not see it performed by the old masters; they see it performed by the men who haven't the skill to do it, and they get the idea that this is a terrible operation, which I do not admit for one minute.

Dr. Francis R. Hagner, of Washington: I am probably very bungling, but recall two instances where I had to operate without a guide, working over one case about two hours and a half. I think a good many supposedly good men have had that experience. In the great majority of cases you can get in and it is the exception where you do not; but there are cases that are almost impossible, and that we have to do a retrograde catheterization, or use some instrument such as Dr. Chetwood's. One of our very prominent members gave a demonstration of that kind. He came to Washington and said that he had never failed to get into a urethra without a guide. In fact, he took a guide and put it into the bladder and took it out because he wanted to show them the operation without a guide. He opened the urethra and worked, and worked, and sweated over it, and finally had to give it up and do a retrograde catheterization on the patient to get in. That man was just as much convinced that he was always able to get in as my good friend here.

Dr. Edward L. Keyes, Jr., of New York: Since my honor has been assailed, I beg leave to plead guilty to that particular brilliant demonstration, but I am now an adherent of the methylene blue injection.

Dr. Hagner: I have used the methylene blue staining, and I have used the fuchsin, and I believe that the fuchsin is better than the methylene blue. If there are any false passages in the urethra the urethra takes the fuchsin stain and the false passages take it in a deeper color. I have used that on several occasions.

Dr. James Pedersen, of New York: Following Dr. Gibson's line of thought, and supposing a case of retention due to (apparently) impassable stricture, why is it not good practice to aspirate the over-distended bladder, put the patient to bed, limit the quantity of fluids taken in, give a saline cathartic, and resort to hot irrigations of the urethra? Thus is obtained a period of about twelve hours during which, as a result of the treatment, the swelling in and about the stricture subsides. One is much more likely to succeed in passing a filiform guide now than he would have been twelve hours before. If not successful in this, how-

ever, a retrograde operation by Dr. Chetwood's excellent method should be performed.

Dr. Chetwood: As I tried to show in my few remarks, this is essentially an operation for special cases, and is not needed by dextrous operators who can always find the urethra without resort to such an expedient. But I think, sooner or later, one is likely to find a case where he is going to have great difficulty and although perhaps eventually he will find the urethra, it is at the expenditure of a great deal of time. This is a time-saving device. It amounts to no more than aspirating the bladder with a large aspirating needle. After aspirating the bladder and introducing the little telescope, the ease with which the outlet is viewed, reveals the practicability of the remainder of the technic. It opens up also further possibilities. One of the recent cases was a case that had been prostatectomized by another surgeon. The suprapubic wound did not heal up and the patient was not able to pass his urine through the urethra, nor was the operator able to introduce a sound into the bladder. He could get only as far, and the sound would halt near the neck of the bladder. Under local anæsthesia, after washing out the bladder, we put in this little instrument, and there we saw a veil of adventitious tissue over the internal meatus, and thus was afforded us an insight into the condition that existed.

In another case of retention of urine simulating prostatic hypertrophy we introduced this instrument, and there was plainly revealed the contracted and elevated orifice of a contracture of the neck of the bladder.

And finally in another case which seemed to be a linear stricture, through which we were not able to introduce any instrument, we were able to pass our filiform guide from behind, and over that the Maisonneuve urethrotome knife to slit the strictured ring. A catheter was left in the bladder through the suprapubic puncture for drainage, and a very rapid recovery ensued.

RUPTURE OF THE BLADDER—CASE REPORT

By J. Bayard Clark, M. D., of New York

THIS case of intraperitoneal rupture of the bladder seems sufficiently interesting to report, for the following reasons: First, that the case was not operated upon until 24 hours after the rupture; second, that the rupture of an apparently healthy bladder occurred without any sign or history of direct injury to the organ.

The patient, a male, 45 years old, of medium height, weighing 194 lbs., was brought into Bellevue Hospital on the afternoon of April 22, suffering from severe abdominal pain, with inability to pass his urine. His past history was negative as to the present condition; his habits were those of a periodic alcoholic.

At ten o'clock in the evening, when called to the hospital, I was told that on admission late in the afternoon the patient was catheterized and 20 ounces of bloody urine were recovered. The temperature at that time was 99, pulse 100, respirations 22. As there was nothing to arouse anxiety and as his real condition was not suspected, he was allowed to remain undisturbed until the evening, when he was again catheterized. This time 8 ounces of bloody urine were recovered and 4 ounces of boric acid solution injected into the bladder, of which only 2 ounces returned.

On physical examination I found the patient with flexed thighs, with a large pendulous abdomen, rigid and tender to pressure over its lower and anterior aspect, with no sign of traumatism, no tenderness in the renal regions; the abdominal walls so thick that percussion was almost of negative value, though there seemed to exist some dulness of the flanks.

The patient's general condition was good, with no sign of shock. He said that on the previous evening after a day of heavy drinking he had slipped and fallen down four or five steps in a sitting posture. He was immediately nauseated and vomited the contents of his stomach. He then started to walk home, twenty blocks away, and though seized with severe abdominal pain, he was able to reach his home, undress and go to bed. After a sleepless night, due to continued abdominal distress, he rose, dressed about noon and requested that an ambulance be sent for, and he reached the hospital, as previously mentioned.

Without further urinary examination the patient was prepared for operation. The usual suprapubic exposure showed a bladder extraperitoneally intact. The incision was carried upward to the umbilicus, in order to get sufficient exposure through the unusually thick abdominal walls; the abdominal cavity being opened, a considerable quantity of urine and blood clots were mopped out. The patient was then put in the Trendelenburg position and the abdominal contents packed off with pads, and a jagged transverse rent in the roof of the bladder, easily admitting three fingers, was found. This being the extent of the injury as far as could be seen, immediate repair was undertaken, and the rent closed by three layers of catgut sutures, placed successively through mucous membrane, muscular and peritoneal coats.

The abdominal cavity was then closed in the usual way, with a cigarette drain and a catheter retained in the bladder. The patient sustained practically no shock from the operation and the post-operative course was uneventful, except for three days of uncomfortable abdominal distention and a slight fascial infection, due to faulty chromic gut. The retention catheter was removed after eight days and the patient was catheterized for a period of three or four days, after which he was able to pass his urine easily. He was out of bed on the the fourteenth day, and at the time of mak-

ing this report, the nineteenth day after operation, he seems perfectly well and strong.

In referring to the literature it is of interest to note the fall in mortality of operations for intraperitoneal rupture of the bladder. From an analysis of 93 cases collected by Watson and Cunningham up to 1906, the mortality per cent was 42.2. According to Thompson-Walker, an analysis of 78 cases gave a mortality of 43.5 per cent, and when only the last six years were taken, the mortality fell to 20.5 per cent. The earlier the operation is performed the better the prognosis. The longest period of delay, when operation resulted in recovery, seems to be that of Quick, who reports a case in the Annals of Surgery, 1907, of a patient, who was not operated upon until 254 hours or almost 11 days after injury.

A factor in making the mortality higher than statistics would suggest is, that probably a large number of single cases, which are operated upon and die, are never reported, as against the successful cases, which the surgeon takes pains to record.

Second Day, May 16th, 1914

THE LOW TABLE POSITION AND OTHER AIDS TO PERFORMING SUPRAPUBIC PROSTATECTOMY

By C. L. Gibson, M. D., of New York City, from The First Surgical (Cornell Medical) Division of the New York Hospital

IN common with many operators I have abandoned the perineal prostatectomy which I practised almost exclusively for ten years to return to the suprapubic operation which I had used fifteen years ago. The earlier suprapubic operations gave me a much larger mortality that the subsequent perineal operations, and there is no doubt in my mind that there is distinctly less shock, generally speaking, in the lower route. The various modifications and improvements of the modern suprapubic operation have made it nearly as safe and the operation gives much better final results.

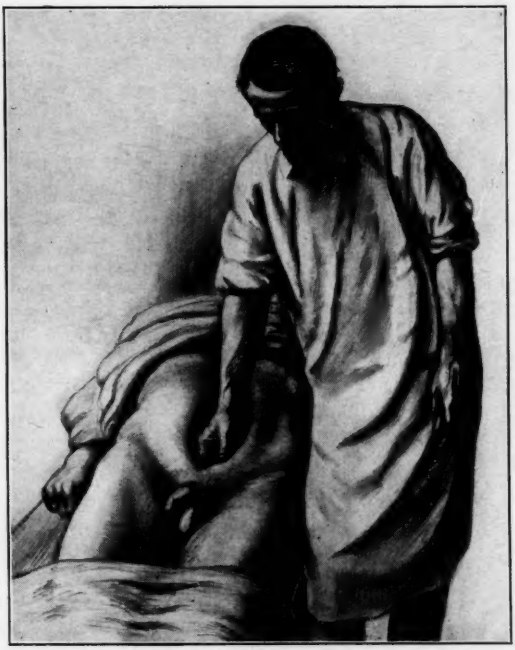
The operation as I do it now is simple, time-saving and is distinctly less risky than when I began my prostatic work. It has four features which I think are all important in attaining these better results—the special position, the anæsthetic, rapidity of operating, and more efficient after treatment by improvement in the method of drainage.

Position

This particular feature gives me a pretext for writing this paper and giving it its title, "The Low Position."

Instead of using the Trendelenburg position, I not only have the patient in the horizontal position, but also use a

PLATE XIII.—Presented by Dr. C. L. Gibson



The operator leans on the patient, depressing the abdominal wall and overcoming the imperfect relaxation entailed by the gas-oxygen anesthesia.

special table 24 to 26 inches high. The advantage of such a position was revealed accidentally. I opened a patient's bladder in his bed under nitrous oxide anæsthesia to drain a bladder filled with clots. To my examining finger the prostate seemed so easily accessible that almost involuntarily I enucleated it in a few seconds time, the very elderly patient making a prompt recovery. On analyzing the features of the operation I become convinced that this unexpected case was no accident, but due to a mechanical advantage not existing ordinarily. By this low position one brings in leaning downward the weight of part of the operator's body on the abdominal wall which being so totally depressed also allows of the easy working of the enucleating fingers in the bladder without loss of force.

Anæsthetic

It is, however, in combination with the anæsthetic of choice—nitrous oxide and oxygen—that the greatest advantage of this overcoming of the muscular resistance becomes manifest. The anæsthetic does not have to be pushed to get complete relaxation—sometimes impossible to get without ether or chloroform.

"Gas oxygen," therefore, under these circumstances, is I believe the best anæsthetic of all. Ether or choloroform I bar absolutely, certainly for the "bad risks," and spinal anæsthesia presents no advantages over gas oxygen and has distinct psychic drawbacks. The effectiveness of the gas oxygen is much increased by giving a hypodermic of morphine, one-fourth or one-third of a grain, about an hour before operation.

Operation

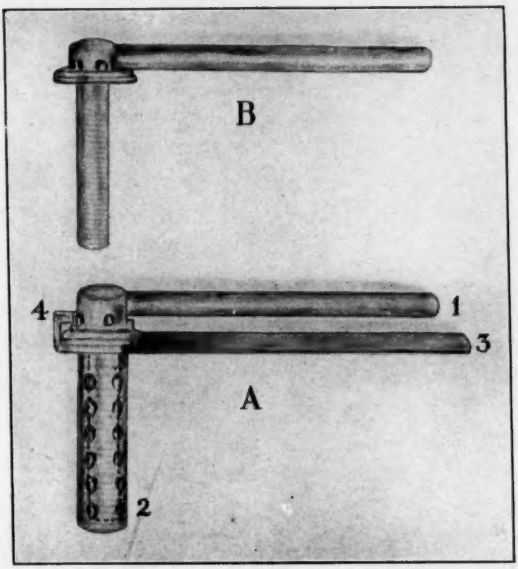
The operation is performed as follows: The patient is placed on the low table. In most cases by omitting one or two scheduled catheterizations, the bladder is sufficiently distended. Any necessary distention (and not much is

necessary) must be done by injecting water, as air distention is unquestionably fatal at times. The laughing gas and oxygen anæsthesia is administered by an unquestioned expert. The skin is painted with one coat of fresh iodine. A mid line incision three to four inches long is carried down to the recti muscles which are separated by blunt dissection. Retractors expose the distended bladder. At its upper position a suture is introduced on either side through the prevesical fat and the whole thickness of the bladder wall (no stripping back of fat or peritoneum, i.e., mangling of the tissues). The bladder is opened between these traction sutures of chromic catgut. An opening admitting two fingers snuggly is sufficient. Bladder palpated, stones removed if present.

The enucleation is easily accomplished by following Squier's procedure, introducing the index finger into the urethral opening and penetrating into and through the thin layer of prostatic tissue overlying the urethra. Having arrived at the outer surface of the prostate it is easily shelled out of its capsule piecemeal or sometimes whole when not large. When one has distinctly struck the line of cleavage the procedure is most rapid and the bleeding is slight. Care must be taken that no pedunculated bits of prostatic tissue or strips of mucous membrane are left to fall into the vesical outlet as a ball valve obstruction. A rubber tube about eight inches long, interior caliber one inch, is inserted just clear of the floor of the bladder. The original traction sutures are now tied and they will subsequently close the opening around the big tube to make further suturing generally unnecessary. While the abdominal wall is being sutured the sucker has been introduced into the bladder through the short wide drainage tube. It is not discontinued till the patient is actually removed from the operating table.

The patient is now transferred with all possible dispatch to his bed where a similar apparatus should be coupled up

PLATE XIV.— Presented by Dr. C. L. Gibson



A. Double metal tube for continous suction in post-operative treatment. 1. Inner tube with corrugated tapering end to be connected with rubber tube from the suction bottle. 2. Short fenestrated outer tube with rounded end to be inserted in the wound. Dotted line shows the position of inner tube. 3. Thin strip of malleable metal, continous with outer tube, which can be bent to fit the surface of the body. It is held in place upon the skin with strips of adhesive plaster. 4. Hook-like projection on outer tube overhanging flange on inner tube holding the two together. B. Inner tube removed showing flange with notch which fits under the hook shown in A. 4.

in readiness to be introduced into the drainage tube so soon as the patient returns to bed. If this procedure has been properly carried out the after care will present a minimum of trouble. The bladder is kept at all times free from blood and therefore from the annoyance that comes from the formation of clots. In forty-eight hours any notable bleeding will have ceased, the large tube will have outlived its usefulness and may be removed, suction still being kept up by attaching a small sized catheter to the sucking apparatus. This catheter is introduced a short way into the vesical lumen.

After Care

The patient should be made to drink large amounts of water. If the intake does not seem sufficient or the patient shows any untoward manifestations such as shock or uræmia, continuous instillation of water in the rectum by the Murphy drip will bring great improvement in his condition. The patient should have suitable nourishment in generous quantities, should be propped up in bed and frequently urged to change his position. Within a week, sometimes less, the patient may be allowed out of bed for portions of the day. Meanwhile, the suction apparatus, after its final withdrawal from the bladder, say in eight or ten days, is applied at the later stages to the surface of the wound thus keeping the patient perfectly dry.

The suction apparatus which contributes so much to the patient's comfortable and successful convalescence is the one devised by Kenyon and Pool (Surgery Gynecology and Obstetrics, December 1909). They say, "To procure the required suction. . . . . .simplest and most practical device is one in which the suction is produced by water or steam on the principle of the Sprengel pump. A serviceable apparatus of this type is the H. D. ejector made by Hayden and Derby

Co. This model can be operated by water or steam at any pressure over 20 pounds and may be connected with a water or steam pipe whenever convenient."

Plate XIV shows some tubes for suprapubic drainage to be used in connection with the suction apparatus. They have also been devised by Kenyon who has kindly furnished me with the illustrations and drawings.

A NEW METHOD OF CONTROLLING HÆMORRHAGE AFTER SUPRAPUBIC PROSTATECTOMY

By F. R. Hagner, M. D., of Washington, D. C.

ALTHOUGH some genito-urinary surgeons whose opinion I respect greatly, consider it is not necessary to use any means to control hæmorrhage after a suprapubic prostatectomy, I know there are others who feel just as strongly that it is necessary to use something to control bleeding. We know these patients are old and many are in such debilitated condition, I cannot help but feel that our mortality records will be lower if we use some method to control this hæmorrhage.

It is not necessary that the patient should bleed to death for the hæmorrhage to be harmful; old, debiliated men losing blood over several hours, thus lowering their resistance, have less chance to recover. Futhermore, my own peace of mind is to be considered for if I have my pneumatic bag in the patient, I am sure that he is not going to bleed.

Pneumatic pressure is not a new idea, as it has been employed before; we know that for years an ordinary condom inflated with air was employed as a pressure agent to check nasal hæmorrhage. We have used various forms of packing in prostatectomy cases, and until the advent of this little pneumatic bag we had used an arrangement of gauze and catheter, similar to that used by Dr. Lower of Cleveland.

Disadvantages

The troubles with gauze in controlling hæmorrhage are (1) that it does not always control it, necessitating repacking; (2) that the pain often caused in its removal and (3),

that secondary hæmorrhage produced by removal of the gauze, although this latter is infrequent.

This little bag overcomes all these obstacles and is a great time saver. I believe it is more comfortable to the patient than the presence of gauze packing.

We tried this bag first on March 3, 1914. We have used it on 10 patients, only one complaining of the presence in the urethra of the tube; this man had an exceptionally tender urethra.

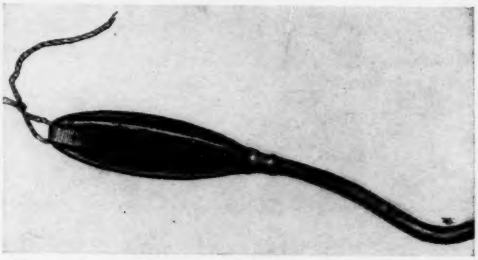
You will see from the drawing that the tendency of this bag when in place, is to accomplish what Dr. Lower of Cleveland, and Dr. Walker of Baltimore, have felt necessary—namely, the pushing of the capsule inwards toward the urethra, not to fill up the cavity and push it back into the bladder.

The bag, made of rubber, is constructed so as to fold automatically when deflated. A rubber tube is attached to the open end of the bag. To the other pole of the bag is attached a small rubber loop to which a string is tied to facilitate its removal through the suprapubic wound. We have also had made a bag with an inlet and outlet tube so that it may be distended with hot or cold water, but the simpler bag controls the hæmorrhage so perfectly that we have found the more complicated bag unnecessary.

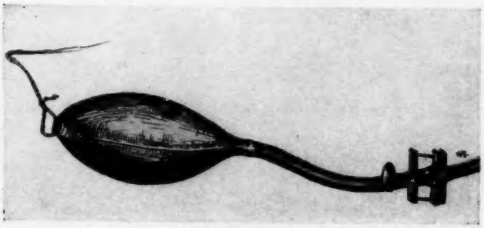
The following is the method of application: A specially constructed sound with beveled end (see Plate XV) is passed into the urethra and out through the suprapubic wound, the tube attached to the bag is threaded over the beveled end and the sound withdrawn through urethra—the tubing being pulled through and the bag pulled into place by traction on the tube. The bag is then distended with air by Janet's syringe, the pressure within the bag being readily discernible by the back pressure on the syringe. It is well to pass the finger into the bladder through the suprapubic wound and sweep it around the bag where it comes in contact with the orifice

PLATE XV.—Presented by Dr. F. R. Hagner

Sound for drawing tube through urethra



Bag deflated



Bag inflated

of the urethra to insure its being in proper position. This also adjusts the mucous membrane at the urethral margin.

The air is retained in the bag by clamping or tying the tube that remains outside the urethra.

To help hold the bag in place we have wrapped gauze around the tube where it comes in contact with the end of the penis, however, the cut off muscle tends to shut down on the tube and hold the bag in position.

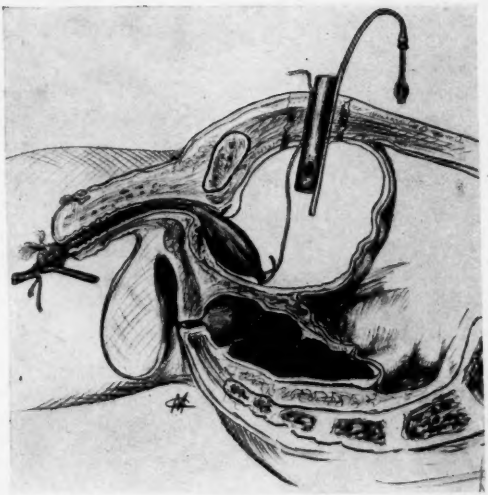
In the 10 prostatectomies above referred to there had been no filling of the bladder with clots. If continuous irrigation is used, the fluid will come away clear, the bag may be put in place and bladder irrigation will show a good deal of blood escaping; but just as soon as the bag is blown up and pulled into place, this hæmorrhage ceases immediately. These patients are all recovering or have recovered from the operation.

In one case the action of the bag was quite striking, the man was in a very precarious condition, having had serious hæmorrhage from the prostate before operation. The prostate was very large and appeared very vascular—the hæmorrhage was great, the pulse going from 90 to 150 in the 5 minutes required to remove the gland. When the bag was put in place the blood was flowing rapidly from the suprapubic wound—as soon as the bag was put in position, the active hæmorrhage ceased and after the clots were washed out, the irrigation returned not even blood tinged. Dr. Squier of New York, has used this bag on one case and informed me that the hæmorrhage stopped immediately following the drawing of the bag into position. At the end of 24 hours we have let the air out of the tube and at end of 48 hours the bag has been removed—in one case the air was let out in 12 hours and the bag removed at end of 24 hours.

It may be necessary to keep the bag distended only a few hours after which, if hæmorrhage should recur, it would be very simple for an assistant to blow up the bag and pull it into position.

The removal of this bag is accomplished without inconvenience to the patient, quite in contrast to the removal of gauze. Suction is made on tube to insure complete closure of bag before its removal; the urethra along side of tube is irrigated with boric acid, the tube hanging out of urethra is carefully washed with soap and water followed by bichloride solution and sterile water. Oil is injected into the urethra and the tube outside the urethra is well anointed with sterile vaseline; traction is now made on the string that is attached to loop of the bag, the tube hanging out of urethra is fed into the urethra as is a catheter and the bag is removed through the suprapubic wound.

PLATE XVI.—Presented by DR. F. R. HAGNER



Section showing bag in place after removal of the prostate.

SOME THINGS THAT INFLUENCE THE MORTALITY AFTER PROSTATECTOMY

By Arthur L. Chute, M. D., of Boston

IT is useless, in my opinion, to deny that the death rate following prostatectomy is still high except in the hands of a relatively few people. If anyone questions the accuracy of my statement, I would ask him to peruse the reports of some of the large hospitals. I will admit that some of these reports are very difficult to translate, that others are so filled with lists of donors and graduate nurses that there is no room for statistics of the medical work, but there remain other reports from which the surgical mortality can be computed. In the last reports of three well-known institutions the mortality following prostatectomy varied from nine per cent to twenty-six per cent—not a low mortality by any means.

Another thing that also points in the direction of a high mortality is the attempt to devise new ways to control hæmorrhage that we have been hearing of lately. Unless men lost cases in which they felt hæmorrhage had played an important part, they would not concern themselves on this score.

While mortality is not the only thing to be considered in cases of prostatectomy, I believe none will dispute me when I say it is the most important single consideration in dealing with these cases. Certainly unless we succeed in the prime essential of saving our patients' lives, we have accomplished but little.

The basis for the conclusions set forth in this paper is a series of fifty-eight consecutive prostatectomies that I have

recently looked up with but a single death. You may say with considerable reason that fifty-eight cases makes too small a number from which to draw conclusions, since, in so small a series, a happy outcome or two or the reverse will change the percentage of mortality materially. It is my feeling that the low mortality in this series of cases depends, not upon chance but upon the recognition of the great cause of mortality in these cases; that much the same things apply to this series of cases that apply to other series of equal size. In other words, that this was an average series of cases and that application of the same methods should give practically the same mortality in other series. These cases extend over a period of two years and have been done in hospital and private practice; a number even in the country where their after care had to be entrusted to men not particularly familiar with the care of such cases. I take the cases of this period for the reason that about that time I began to recognize what I now believe is the underlying, fundamental point on which mortality for the most part depends in these cases of prostatectomy.

To come to the point at once, I believe that the danger in prostatectomy lies almost wholly in the renal condition; that with good kidneys properly functioning, or even with rather poor kidneys that have been gotten into good functional condition, these patients get well in the vast majority of instances; one might almost say regardless of operation or operator. When, however, this principal danger—the renal condition—is not recognized or not attended to, we have a moderate number of patients who "peter out" from two to five days after operation and are supposed to die of shock, hæmorrhage or exhaustion. It is my opinion that the vast majority of these patients really die of renal insufficiency, though they do not show signs usually considered to indicate this condition. At any rate, with the functional condition of the kidneys carefully looked out for, these apparent deaths from shock, exhaustion and hæmorrhage do

not occur with anything like the frequency that they formerly did.

There are two renal conditions that may endanger the life of the patient who is about to submit to prostatectomy. The first is the renal suppuration, pyelonephritis. In these cases, when the kidney involvement, plus the degree of temporary disability due to back pressure bring the patient's renal reserve down pretty low, the irritation of ether will often cause a fatal shutting down of the kidneys, which, because of its relative slowness, leads to the idea of death from exhaustion. If one has them in mind, these pyelonephritis cases are relatively easy ones to recognize and to guard against because they present an infected urine of low specific gravity, a distended bladder, and in the more advanced cases, digestive symptoms. In these cases it is easy to judge the amount of renal disability by estimating the phenol-phthalein output, the quantity of urea, the amount of albumin and such things.

The other type of case that is especially dangerous is the one where one has a chronically over-distended bladder with non-infected urine. I have called this condition one of renal embarrassment and referred to it in a paper entitled "Some Observations on Cases of Prostatic Obstruction Presenting Over-Distended Bladders," that I read before the Philadelphia meeting of this society two years ago. These cases are much more dangerous than the others for two reasons: first, because they are very often overlooked on account of the lack of symptoms pointing to the kidneys; second, because they have not received the immunity granted by a chronic infection and almost any manipulation will be attended by infection. What these patients may show is simply a low gravity urine in large amount, usually with, but occasionally without, a little albumin. This large amount of low gravity urine is evidence of Nature's attempt to rid the organism of the usual amout of waste products with kidneys that are unable, because of the embarrassment of back

pressure, to filter solutions of the usual concentration. The kidney's situation is not unlike that of a water-wheel which is partially or wholly put out of service because of the level of water below the dam rising to the point where the wheel does not clear itself. Both water-wheel and kidney have plenty of potential power which they could exert under more favorable circumstances.

These patients may have coated tongues and dyspepsia, and if the process is advanced, nausea, or even vomiting; but no œdema, no headache, no blurring of vision, rarely renal elements in the urine. The blood pressure is not necessarily high. These patients are in condition where an ill-considered anæsthetic may cause their kidneys to shut down. Following it they become dull and heavy, their tongues become dry, their abdomens often distend, and they gradually fail, dying in from two to five days of symptoms that it is easy to attribute to shock or hæmorrhage if they come early, and exhaustion if they come later. The falling blood pressure seen in the latter part of the course of these cases, in conjunction with the greater amount of staining that we see in general with the suprapubic operation, would seem to add color to the probability of death from hæmorrhage. Such deaths are, I believe, really due to renal insufficiency. At least, cases which start in this way may occasionally be gotten well by stimulating their elimination, sometimes even when they have gotten to the stage of soft distention and vomiting, and cases whose condition is similar may be prevented from developing such symptoms by proper pre-operative treatment. I have yet to see one of these patients die who was passing 80 to 100 ounces of urine daily. In the fatal cases, the quantity of urine, when definitely known, is always small.

As there is no point that we can say definitely indicates the danger line, I feel we should treat all prostatics with a residual urine as though their kidneys were under suspicion. The very least we can do is to keep these patients under ob-

servation for several days prior to operation and make out definitely the quantity of urine and its specific gravity. If there is any indication that the renal function is not perfectly good, we should not consider operation till the kidneys have been gotten to function well.

It is upon the whole clinical picture that the decision of the necessity of bringing up the renal function must be made, and not upon a single sign. One does not like a low phenolphthalin output; 30 per cent in two hours, for instance, makes me apprehensive; a urine that has a constant low gravity and a slight trace of albumin is bad; a very large amount of low gravity urine is dangerous; worse than these things is a patient who has, in addition to the above, digestive symptoms in the shape of a coated tongue, lack of appetite, nausea, or vomiting. A high blood pressure, especially if there is but little evidence of sclerosis of the superficial vessels, is ominous. These are points that make me particularly concerned, but all patients with a much distended bladder are risky, especially if the bladder has been symptomless. This is because the lack of discomfort means a lack of muscular tonicity of the bladder with probable dilatation of the ureters and embarrassment of the kidneys by back pressure. Patients who present uninfected urines are less favorable in a way than those who have been infected, since this latter class have developed a certain immunity which the former do not possess.

These prostatic cases with failing renal compensation or threatened failure of renal compensation are most satisfactory to treat—wherein they differ from the failing renal conditions of the medical type, where there is little to be done except to take off as much work as possible and perhaps drive the heart a little harder. We cannot in the *chronic* medical conditions hope for any real, lasting improvement in the condition of the kidneys. With these chronic back pressure cases, however, we may confidently expect a restoration of function that is equal to the amount of crippling that

is due to the element of back pressure. This often amounts to enough to make the difference between passing through a prostatectomy or succumbing to it. One sees cases where, before operation, the specific gravity had been about 1010 or less for a considerable time and where, some months after operation, the same patients pass a urine of a gravity of 1020 or more; showing a great gain in the kidneys' ability to excrete a concentrated solution of waste products. This is chiefly if not solely due to the relief of back pressure. The difference in these two conditions represents the renal disability that had been brought about by back pressure.

The principle to be followed in these cases is to relieve completely the back pressure. This is most definitely accomplished by opening the bladder suprapubically, and putting in a large sized tube. I feel that this operation should be done practically always by means of local anæsthesia, preferably novocaine. I have used spinal anæsthesia for this purpose in a few instances, but do not do so routinely because it is unnecessary. In my opinion, ether must not be used, for if a patient's renal condition is sufficiently serious to require this manœuvre, he ought not to be subjected to ether because of its irritating action on the kidneys. My experience with chloroform and gas-oxygen anæsthesia for this purpose has been nil—but I doubt if they are so desirable as novocaine. It is in cases where the bladder is over-distended with non-infected, low gravity urine that this preliminary drainage by suprapubic incision made under local anæsthesia, receives the most urgent indication.

As a rule the ureters are dilated in these cases. I know of no way that one can catheterize such a bladder for any length of time without infecting it, and that, too, relatively early in the process. With the bladder infected, the intermittent backings up of urine between the periods of catheterization, if this is done at intervals, or at such times as the catheter blocks if constant drainage by catheter is attempted, allows the infection to extend up one or both

ureters. We then have a pyelonephritis, the exent of which will vary, as will also its seriousness. Our best insurance against this is a large suprapubic tube, which will not allow of back pressure. In spite of this precaution, the infection of the bladder which is bound to occur, may extend up a ureter; but we have done our best to prevent it, and the time before infection takes place may be sufficient for the ureters to contract and reassume their protection of the kidneys. If the patient's urine has been infected for some time and there is no appreciable amount of temperature, the need of this preliminary suprapubic drainage is less; presumably infection of the kidneys has already taken place if it is going to occur, and a certain immunity has been developed. If, in such a case, we can keep the bladder empty by means of a catheter, and the retention catheter is well borne, the requirements for the relief of the back pressure are sufficiently well met. If, however, there is a turbid urine with temperature and perhaps renal tenderness, immediate suprapubic drainage may limit the extent and damage of the acute renal infection that has probably just taken place.

There is one incidental advantage of the preliminary suprapubic incision, even in the cases where it is not urgently indicated—it allows us to wall off the space of Retzius before the time of the chief operation; it causes a diminution in the congestion about the prostate with consequent diminished liability of bleeding,—a prophylactic treatment of bleeding that I believe is desirable.

How long a time is required, after drainage of the bladder has been established, either by suprapubic incision or by retention catheter, to get the kidneys into condition to do the prostatectomy? This varies widely with the case, also with the sort of anæsthesia one proposes to use for the operation of enucleation. I have almost always waited a week and more often two—on one occasion recently I waited between ten and eleven weeks. One should wait until the patient is passing a good amount of urine and until his tongue

has cleared up. If there is distention one should wait till this has disappeared. In other words, wait until all signs of urinary intoxication have ceased. If one is to use ether for the enucleation, I believe it is essential to wait longer than if one is to use spinal anæsthesia. There is no limit to the time one may wait. I have, on a number of occasions, seen patients in the country in serious condition from renal insufficiency due to back pressure, have opened up their bladders under local anæsthesia, and then left them for some weeks. At the end of this time I have either had them sent to me in town for the completion of the operation, or have done it at their homes.

The only objection to this procedure is the fact that the preliminary suprapubic drainage rather commits one to the suprapubic enucleation of the prostate. I find that I still prefer to do two-fifths of my prostates by the perineal route. The small fibrous prostate, the clearly malignant prostate, or the prostate that suggests probable malignancy, the prostate in the very fat man—all these I believe the man who has acquired a reasonable facility with the perineal operation will prefer to do by that method. The suprapubic opening is, of course, no absolute bar to carrying out the perineal operation.

Having, by preliminary drainage, gotten the kidneys of a prospective prostatectomy patient into good condition, it is essential that we do nothing that may injure them. I believe ether is a great renal irritant, especially to these kidneys that are convalescing, so to speak, and avoid it if I possibly can in patients whose renal function has been poor. In its place I use spinal anæsthesia with increasing frequency. I have used it forty times in this last fifty-eight cases. While I cannot say that I have never had disquieting experiences with it, I have never had a fatality and use it with growing confidence. The occasional threatening episodes during or following spinal anæsthesia are preferable

to the euthanasia that I believe ether not infrequently brings about with these patients.

Bleeding, which has, I believe, received more attention than it deserves, and is entirely secondary as a cause of mortality to the renal condition, comes in for consideration more especially in connection with the suprapubic operations. It may be that hæmorrhage may be an important factor in the bladders that are distended with water by use of the constant irrigation apparatus following prostatectomy, due to the effect that the hydrostatic pressure may have in keeping open the mouths of bleeding vessels. I have always felt that it was unnecessary to control the bleeding by this method, and have never employed it. I felt that it was pernicious in that it did not allow one to measure the amount of urine secreted—a thing that must be known in order to guide accurately the patient's convalescence immediately following operation.

I see patients at times who ooze very freely; once in a while I see them bleed more than I believe is good for them, but even then they do not die so long as they are passing a large amount of urine. Therefore I doubt if fatalities often depend upon excessive bleeding. While I am careful to avoid excessive bleeding following operation, and have packed two, I think, out of this list of fifty-odd patients, yet I cannot help feeling that moderate bleeding is more likely to be beneficial than detrimental, especially in the men who have a high blood pressure at the time of operation. I believe that relatively few patients bleed enough to do themselves great harm; that bleeding is rarely a cause of death following prostatectomy. I believe that the time spent in the complicated ways of arresting hæmorrhage is worse than wasted. It prolongs the time of an operation, which one should make reasonably short; it also adds to the trauma. The importance of these last two elements is of course less when one operates under spinal anæsthesia, but is, in my opinion, important to avoid.

A method that I have employed when I have had a troublesome bleeding in suprapubic cases—one that takes but a few moments and has always proved effective—is to pass a good sized soft rubber catheter, say number 18 or number 20 French, into the bladder by way of the urethra. A styletted catheter may be necessary if there is difficulty of introduction. This catheter is sometimes fastened in place by a stitch through the meatus, but preferably by bringing its tip out the suprapubic wound and by passing a silk stitch through its end and then passing this stitch through the edge of the suprapubic incision. The stitch is tied with enough slack so that the eye of the catheter can be withdrawn well down toward the bladder outlet; then, with the catheter held securely in place, the prostatic cavity is firmly packed with gauze strips around the catheter as a core. This apposes the funnel shaped process or diaphragm, that is sometimes left at the bladder outlet after an intra-urethral enucleation, to the sides of the cavity from which the prostate has been removed. This method has, in my hands, never failed to control bleeding promptly and efficiently. It can be done rapidly and by touch without seeing the field. Dr. Lower[1] has in a recent article, described this method as a procedure which effectively *prevents* hæmorrhage. I have never made use of it as a prophylactic measure, but believe it is better than the more elaborate measures if one is going to make routine use of some method of hæmostasis.

If my contention is right, the high mortality following prostatectomy is unnecessary and is to be avoided in two ways, first by getting kidneys that are not doing their work into a condition where they are acting efficiently; second, by avoiding injury to embarrassed or susceptible kidneys at the time of operation. According to my belief the question of the mortality can be and has, in most instances, been settled, before one begins his prostatectomy. This question hinges almost wholly upon the condition of the kidneys, or perhaps, to speak more accurately, upon the functioning of the kid-

neys. The task of getting them into condition must be done before operation. A certain amount can be done after operation, but for the most part whether or not a patient is going to recover has been decided before we begin our prostatectomy, and depends upon the renal function.

REFERENCE

[1]William E. Lower: "A Technic for Performing a Shockless Suprapubic Prostatectomy," Annals of Surgery, February, 1914, p. 279.

DISCUSSION

Dr. J. Bentley Squier, of New York: It is not at all necessary to back up any statement of Dr. Hagner's in this Association. When he makes one we know that everything he says is absolutely correct. I merely want to add a little additional testimony, however, to the fact that he came over to New York during the embryologic period of that little pneumatic bag, and he allowed me to use one, and it does everything that he claims for it.

Dr. Louis E. Schmidt, of Chicago: We are obliged to admit without question that hæmorrhage is probably the most important condition that we meet with in prostatectomies, and every effort that can be made to control it, should, I think, be tried out. I might say in the past few months I have tried fat implantation into the cavity of the prostate by removing fat, taking a piece as large as the size of the cavity, and sewing it right down tightly into the cavity. Of course, this has been used a good deal elsewhere, in general surgery particularly, but I have also used it in connection with kidney work. As far as my experience is concerned it is too limited for me to be able to express any positive opinion, but the results have been so marvelous in general surgery that I thought it desirable to use it. I have also sewed up not only the edges of the bladder, with apparently the capsule, and controlled it in that manner, and also the capsule itself. There is no question that all these methods consume an enormous amount of time, and this method of Dr. Hagner's appeals very strongly to me, and I certainly will take advantage of what he has told us today, because I firmly believe that up to the present time it seems to be the quickest and from what he says is almost absolutely certain to give enough pressure and in addition to that does not give the discomfort that packing is known to give. Anyone who has had much experience with packing knows the enormous amount of pain and discomfort that it produces, and of course all the disadvantages of removing it, and particularly the secondary hæmorrhages that may occur. This idea of allowing the bag to remain in from twelve to twenty-four hours and then again inflating with the greatest of ease is certainly something that no other method has given us.

Dr. C. L. Gibson, of New York: I wish to call your attention to these various forms of ingenious devices for applying suction to the bladder. These things have been known from time immemorial. The

people who started them were the dentists. I think Dr. Chetwood has been one of the most recent inventors. I have been using the apparatus ever since it was brought out by Dr. Kenyon and Dr. Pool. From time to time they have improved the apparatus somewhat.

In this operation I have the suction apparatus all ready to put in action, and the patient is transferred from the operating table to the bed and as soon as we can get there we attach this suction apparatus. It is my custom to use a very large tube as a very temporary procedure. The tube is withdrawn in from two to four days. Then a small tube is put in. Getting the big tube out early allows the incision in the bladder to collapse much more readily. We nowadays do not see any more of that infection of the prevesical space which used to be a regular complement of that operation of suprapubic cystotomy.

Dr. Charles H. Chetwood, of New York: Apropos of Dr. Gibson's paper, I have long been interested in developing a method of efficiently accomplishing the suction of the bladder after all suprapubic operations, and have reported upon the subjcet before this Society on previous occasions. I have used the water suction, the so-called laboratory pump that Dr. Gibson demonstrated, and found it more efficient than anything else I had used before, but water pressure is not always the same and when it varies, sometimes the water pump works and sometimes it does not. If you are in a large institution where you have a tank on the roof and the water pressure is stationary it works satisfactorily, but it does not always work the same way in different places. So I had developed an electric suction pump which I demonstrated before this Association last year and which I have continued to use with great satisfaction, because the pressure can be set and kept at a certain point. This apparatus can be put in a ward and used upon half a dozen patients at one time. It may be set when the patient is convalescent for suction every five or ten minutes, later every twenty minutes or a half an hour, and automatically works for the time it is set, as you like. The appartus is attached to any electric outlet.

I think that Dr. Hagner's device is an exceedingly valuable one, and I agree with others that we do have cases in which the danger of hæmorrhage is such that we will be much more comfortable in having such a device as this to control it. I think the particular advantage of it is that it smooths down the mucous membrance in the bladder wound against the outlet better than the packing of gauze which does not tend to produce the smooth surface that this rubber bag leaves afterward.

Apropos of Dr. Chute's paper, I think he has sounded a very timely note with regard to the kidneys and their important bearing upon mortality, and brings up again the point that has been spoken of before of the advantage of a two-step operation in those cases in which the kidneys are not able to withstand the immediate trauma of complete operation. By such means the safety of many cases is conserved, especially off in the country where they are not in the most experienced hands and a preliminary perineal or suprapubic drainage, or even an indwelling catheter, to drain the kidneys for as much time as may be necessary, will often compensate for the otherwise unfavorable conditions.

Dr. William E. Lower, of Cleveland: I think this Association is to be congratulated on having three such interesting papers on such an important subject.

Dr. Gibson's device appeals to me especially, because I always have had difficulty in getting above the patient. It had not occurred to me to lower the patient—I supposed that operating tables were standardized and must remain so. This device, however, does not appeal to me for the particular service that he advocates by asserting that he is able in this way to hold his patient throughout the anæsthesia. I think a patient should be so completely controlled, either with a general or a local anæsthetic, that the operator will not meet with any resistance, and that is one of the points to which I wish to call particular attention, i.e.—that it is possible to remove prostates by the suprapubic route without having the patient feel the operation in any way. By "feeling the operation," I mean manifesting it by increased pulse rate or respiration,—an end which can be secured by combining the general with the local anæsthetic,—by using the so-called anoci method of Crile. This can be very successfully done, as I have demonstrated repeatedly, by infiltrating not only the skin, the fascia, and the muscle, but also the prostate and the tissue around the prostate; by using a long needle and syringe so that the deep parts are anæsthetized, and then by using such careful manipulations that they are not at any time resisted by the patient. The aim is to see not how quickly but how carefully the operation can be done.

I think the question of hæmorrhage is one requiring careful consideration. In certain individuals with high blood pressure it may be a good thing to have a little bleeding, but certainly these old people have little resistance, and we have to protect them in every way possible. To take away much blood, therefore, is not a good thing in the majority of cases.

Hagner's device seems to me to be most ingenious and it appeals to me very much for the purpose for which I have been using gauze, that is,—not only to control hæmorrhage, but to bring the two raw surfaces of the capsule down together so that when the gauze is removed, a funnel-shaped mucous membrane is left. If the gauze is well lubricated with sterile vaseline, especially that part that goes through the abdominal wound, very little discomfort will be caused by taking it out, or if it adheres very tightly a few whiffs of nitrous oxid should be given when it is removed for I do not believe the patient should be hurt before, during, or after the operation. The course of the case reported by Hagner does not seem to me to have been due to hæmorrhage. I do not believe that in five minutes' time the pulse rate can be changed from 100 to 150, unless there is very profuse hæmorrhage. I think he was hurting his patient. It was a difficult prostate to take out, and he increased the pulse rate by traumatism. If the change in pulse rate had been due to hæmorrhage the patient could not have recovered from it so quickly.

Dr. Chute's paper has been most interesting, and I think it is quite important. Instead of making a suprapubic incision for a preliminary drainage, I have been making a suprapubic puncture and putting a tube in from above. This can be done under a local anæthetic, and the bladder will be drained without any discomfort and the patient be able to be up and about.

So far as preventing any traumatic impulses from reaching the brain is concerned, spinal anæsthesia, of course, would be the ideal anæsthesia. I have not been using it, however, because I think the mortality rate that it carries is a little too high. I quite agree with him that ether should not be the anæsthetic in these cases. I think it adds to the dangers and should be avoided.

The pre-operative condition of the patient is a most important matter, and I agree with Dr. Chute that we can judge pretty definitely beforehand whether an individual patient is going to recover or not, if he has been carefully examined and the kidney function has been tested. Occasionally, however, we do have some post-operative complications which we cannot anticipate. The renal function is one of the most important factors in these cases.

Dr. James Pedersen, of New York: In discussing Dr. Chute's paper, I should like to call attention to the blood pressure and the pulmonary condition as important factors in estimating the surgical risks that these patients present. I recently lost a seventy-one year old patient

three days after suprapubic prostatectomy. I had under-estimated the significance of his low blood pressure and his obvious tendency to bronchitis complicating an emphysematous condition. Though his physician said that the man did not have emphysema. I had the patient put to bed in the hospital for five days of preparatory treatment before operation. The urine was perfectly clear, and there was no sign of renal insufficiency. My error lay in allowing the anæsthetist to use gas and ether, having in mind his feeble circulation. Chloroform was substituted, but the mischief had been done. Within twenty-four hours the patient had developed a sharp bronchitis of the larger bronchi; close upon that came a further reduction in his blood pressure; the circulation failed; œdema of the lungs followed, and death supervened on the third day. The case offers only another argument for the more regular use of gas and oxygen in the place of either ether or chloroform, under such circumstances. The man certainly did not die because of failing kidneys, but of a circulatory and pulmonary failure.

Dr. Edward L. Keyes, Jr., of New York: I was greatly moved by all these papers. I think that we really ought to congratulate ourselves in having three papers which cover so handsomely three very different and yet important points in the matter of prostatectomy without anybody very violently getting up and insisting to us that the operation which he had discovered was the only thing which preserved the patient's potency.

As a matter of fact, it seems to me it is, after all has been said and done, the operation that kills the patient. You can discuss the relative importance of hæmorrhage and blood pressure and kidney function without ever coming to any more close agreement than that if you had not operated upon the patient he would not have died on precisely the same day that he did die after your operation. Therefore, it seems to me that Dr. Chute is not quite right in stating that the preliminary treatment is the whole thing—that the safety of the operation can be insured by the preliminary treatment. He made one other remark. He said that the question of the dangers in operation was not one of chance. It is not chance; it is very often Chute, judging from your figures, sir. But the preliminary treatment is one point, and the way in which the operation is done is another point. I have seen Dr. Chute operate, and he does a sound operation; a simple, plain operation without attempting any fancy stunts.

Now, in the operation it seems to me that the important preventable dangers during the suprapubic operation—with which I am most familiar

—are infiltration of the space of Retzius, the hæmorrhage and the anæsthesia. Before operation we can do much and should do all we can to improve the patient's circulatory or renal or pneumonic conditions by various treatments, many of which he has enumerated. I confess I am rather turning away from the suprapubic preliminary drainage and depending more on the catheter than I have in the past. I have had the shock of losing several patients from suppuration in the abdominal wall after prostatectomy done secondary to suprapubic drainage. Therefore, I know that, in my own cases at least, the suprapubic drainage does not absolutely prohibit that cause of death.

I am not sure that my choice of anæsthetic is entirely settled, because I am not entirely clear in my own mind as to the fundamental principles on which the choice of an anæsthetic is to be made. No anæsthetic is to me satisfactory for prostatectomy. I know, however, in reference to spinal anæsthesia, which I use a good deal, that the man who has done more spinal anæsthesia in Boston than anybody else, was the most conservative of the various gentlemen to whom I recently spoke in a visit to that town. He spoke as though he was scared to death every time he used it.

In the second place, the rapidity of the operation, of course, is all important.

In the third place, I think the infiltration of the space of Retzius depends very largely upon the use of a double current irrigation through a suprapubic wound. I have done that, and the deaths I have had from it have persuaded me that one should never use a double current irrigation through a suprapubic wound. You are so apt to have the muscles grip around the tube and force the bladder contents out beneath the abdominal wall.

The suction apparatus spoken of by Dr. Gibson and again by Dr. Chetwood I most heartily endorse. I think it is useful both in preventing infiltration and making the healing very much more rapid. My cases do not heal in two weeks as pretty much everybody else's do.

I have seen and studied with interest various methods for stopping bleeding, but the ideal is to pull upon the bladder neck, as our friend Dr. Samuel Alexander used to do in his perineal operation. This bag pulls the bladder neck down. Dr. Lower has done the same thing with gauze. I have attempted to do the same thing with suture.

Dr. Squier: I think we all of us try various methods as they come out. We all of us develop a techinque which is satisfactory to ourselves. In a large experience with this operation I have tried, I think, most of

the methods which the rest of you gentlemen use, and I have come to the conclusion that the patient does quite as well without any such an apparatus, without any post-operative irrigation, as with it, and that is from an experience in prostatectomy of over five hundred operations. At the present time I am perfectly satisfied that the anæsthesia of election is gas-oxygen. I feel that the rapidity of the operation is important, provided a man does it with care. I think you should produce the least traumatism possible, as Dr. Lower suggests. I believe after the prostate has been removed that gauze soaked in a hot solution of some kind left in the cavity for a few minutes is all that is necessary to control hæmorrhage, to suture the bladder around a comparatively small tube and a small drain in the space of Retzius and no irrigation in the bladder for the first forty-eight hours. I have been doing this for the last year or so, and that would include many prostatectomies, and my results have been quite as good as they were at any other time in my career.

In regard to mortality and two-step operations, I have been rather more unfortunate than the rest of you gentlemen. I have done the two-step operation on three patients and I have had three deaths. They were very desperate risks, however. It, however, stands out that my mortality is one hundred per cent. I cannot believe that the suprapubic drainage has very material advantages over the preliminary use of the catheter in those cases. I think that is a thing to be argued by the hour. So I won't argue it any further.

But I do believe in a minimum of anæsthesia and rapidity of operation. I think the patient is sometimes shocked in getting on to the operating table and being removed back to the ward as much as he is during the operation. I think the time will come when we will go into a patient's room early in the morning, give him a smell of anæsthesia, and take out his prostate just as Dr. Gibson demonstrated in his Ambrose Pare attitude. It will be just in line with the man's day's work. We will take it out, get him back to bed and he will be reading his newspaper a few hours after the operation, and that man will get over it.

Dr. Keyes: Let me just say that prostatectomy is not a minor operation.

Dr. Squier: It is not a minor operation, but I think we add a great many major features that are unnecessary—getting the patient on the table, scrubbing with green soap and bichloride, arranging the towels all over him. And after operation, the house surgeon squirting water in and out of the bladder, and while he is doing it you say, "You see, by this

method we have no hæmorrhage." The patient gets back to the ward in what condition? In a condition of profound shock—from the operation possibly, from the rest of it undoubtedly.

Dr. G. K. Swinburne, of New York: I only want to say one thing about the mortality, and that is to illustrate by two cases that happened last fall. Both were eighty-five. One was thin and poorly nourished and anæmic; the other was a fine strong man. The operations were done at about the same time. The man who was so well nourished had a good phthalein output; in fact, he was all that one could desire. They both had the two-step operation, the bladder opened under local anæsthesia, and a week later under a whiff of gas the prostate removed. The man who was strong at the end of three weeks was ready to leave the hospital, and the day that he was going out of the hospital, the nurse found him dead on the floor from embolism or apoplexy. The other man lived. He had subcutaneous abscesses all over, which were opened one after another, and left the hospital nothing but skin and bones, to recover, and he did recover very nicely, but went to a convalescent home for a while.

Dr. Henry H. Morton, of Brooklyn: I should like to ask Dr. Chute if he does not have a good deal of trouble in the two-step operation with necrosis and sloughing of the suprapubic wound. I have done the two-stage operation a few times, but the wound has always sloughed and become infected and some of the patients died of sepsis. Now I depend on preliminary drainage by means of the permanent catheter, or if that is not tolerated, on a perineal section, with a large perineal tube.

The question of hæmorrhage is one that has interested me more since I began to look into the bladder after opening it suprapubically and see how much bleeding there was. When I first began to do a suprapubic prostatectomy, I enucleated the prostate and put a tube in the bladder without looking to see how much bleeding there was, but in the last few months I have been inspecting the bladder by means of retractors and I have been surprised to see how much oozing there was. In several cases there was so much bleeding that I did not think it safe to leave it without a tampon. In a good many cases I have packed the cavity of the prostate with gauze. Using a little trick that I got from some book (I have forgotten whose), the gauze is folded and a tape drawn through it. Then a sound can be passed through the urethra and the tape tied to the end of the sound and the gauze pulled into the prostate. That would not always control the bleeding and had to be reinforced occasionally by packing the bladder. This little arrangement of Dr. Hagner's is cer-

tainly a very good one, and I am going to substitute it for the packing with gauze.

There is one other point that I have found very useful. It seeems to me that it is a mistake to use continuous drainage and irrigation of the bladder. You have a wound, and it seems to me it is a great deal better to leave that wound alone, while it is healing. If there is going to be any trouble it will be announced by a rise of temperature. Then you can begin washing out the bladder and the temperature falls. I should like also to refer to a little dressing that prevents the drainage from getting all over the belly after a suprapubic opening. A rubber dam such as dentists use is put around the suprapubic wound and stuck fast with zinc oxide ointment, the gauze dressing is put in, and the rubber dam is folded up over it, and the urine is received in the pieces of gauze and does not get over the belly, and the belly is kept clean and free from this eczematous inflammation which is so troublesome to the patient.

Dr. Paul Thorndike, of Boston: I want to say a word of commendation of what I have just heard Dr. Squier say. It seems to me this operation is safe because it is simple, easy and quick. The more you complicate it the less safe it is, and I believe that anything in the way of complication, either of the operative procedure itself or of its convalescence, is a source of danger, unless it is imperatively indicated.

I am one of those to whom Dr. Hagner referred whose experience has been such as to feel that hæmorrhage is not a very serious matter, and it has never been my habit to utilize any method for the control of hæmorrhage. I wish to say that I shall never do another suprapubic operation without having Dr. Hagner's little rubber bag ready to use, but I do not believe that I shall ever come to the place of using it in every case.

As for Dr. Chute's two-stage operation, since his paper in Philadelphia two years ago I am sure that I have in one or two cases saved my patient a great deal of risk and myself a great deal of worry and discomfort by resorting to this measure. Here again I do not believe it is a measure that is indicated quite so frequently as Dr. Chute's remarks have led us to suppose, but I do believe that occasionally a two-step operation is wanted.

It is not necessary to say anything about the careful study of the kidneys previous to operation. The importance of that cannot be overestimated, and I have always had the feeling, as has Dr. Chute, that that is the most important single pre-operative thing to consider.

As for the operation itself, I can only say that I think the quicker, the simpler and the less complicated it is, the safer. Personally I do not irrigate the bladder afterwards.

Dr. Hugh Cabot, of Boston: In the first place I am not in accord with laying quite as much stress as Dr. Chute did upon kidney function as compared with that of other organs. I think that of late years we have all of us thought very carefully of kidney function, and there is a tendency, which I believe to be fostered by Dr. Chute's paper, to leave in the background the other big systems—the cardio-vascular and the pulmonary.

My operative mortality in the last year in prostatectomy has been high. Fortunately the cases have been carefully studied, and of the deaths a considerable proportion have come to autopsy. In no single case was kidney insufficiency the cause of death. They died of emboli, they died of acute cardiac conditions other than emboli, and of pulmonary complications, with kidneys in first class condition, and of the cases we have lost, none of them have been the ones we should have expected to lose had we paid attention to the kidney condition, and that in the face of the fact that I think I have been very conscious of the dangers from those other conditions.

Personally, I believe that the cardio-vascular and the pulmonary systems are of prime importance, and they should be watched in two ways: First, I think many hearts can be improved, as to their muscular condition, and second, comes the choice of anæsthetics, which I believe to be very vital. During the last three years I have been at work upon a paper which I am to read in Berlin in a short time upon the choice of anæsthetics in urology, and for that purpose I asked my associate, Dr. Miller, to study very carefully the effect of ether anæsthesia upon kidney function. He studied between 600 and 700 cases, tested carefully before and after ether anæsthesia, and got some very interesting results. which I think I can briefly sketch here without impropriety.

In all cases where ether anæsthesia was used for any length of time, that is to say, from three-quarters of an hour up to an hour and a quarter, there was a drop of kidney function of at least a quarter; the duration of that process was about twenty-four hours, and at the end of forty-eight hours the kidney function had returned to substantially normal.

Then there were some very striking observations on the effects of shock—not operative shock, but pure shock, railroad cases, multiple fracture cases. Those cases show a kidney function depressed to prac-

tically nothing, some that could not be read by phthalein, in the face of a large urinary output. After the shock had passed, in four or five days to a week, after a fracture had been set or the thigh amputated, the kidney function was normal. That bears directly upon the work of Dr. Crile in relation to the effect of shock on kidney function. I believe we must be very careful and cannot be too careful in the elimination of shock. It has led me to utilize spinal anæsthesia to a very large extent. It is of course the ideal method for the prevention of shock, and one will use it or will not use it according as one believes it to be safer than other methods. I believe that ether ought to be almost entirely excluded from the field.

I have been more afraid perhaps than was necessary of gas and oxygen in cases of disease of the circulatory apparatus. Dr. Lower's experience will lead me to use it more. My preference is a short general anæsthesia for the introduction—preferably gas and oxygen—to remove the psychic shock of the spinal puncture, then the removal of the general anæsthetic, and the finishing of the operation under the spinal anæsthesia. This I believe to be a method which eliminates shock and gives good results. The fact that the patient is perfectly comfortable, both physically and psychically, is the best protection not only of the kidney function through the elimination of shock, but this method also protects the cardio-vascular system and the pulmonary system by the elimination of general anæsthetics of any type. One must, of course, modify one's views according to the experience of the moment. If spinal anæsthesia ultimately proves to have a higher mortality than we now believe, it will have to be pushed aside.

I have not been able to get good control of shock by the method that Lower suggests. I have been unable to satisfy myself that I could get anæsthesia in the region of the prostate. It may be due to my unfamiliarity with the method. It is a deep hole, and I am not sure where my needle is going. I incline to the view that the combination of general to start with, then spinal anæsthesia, and then your operation carried along smoothly and without psychic trauma, is probably the most satisfactory anæsthetic medium.

I believe that bleeding is a very important factor in mortality, not because the bleeding is sufficient to immediately affect them, but because it always lowers their resistance, gets them down below their par value, and exposes them to accidents, cardiac, pulmonary, or renal. I believe the careful control of hæmorrhage is very essential to good results.

I am much impressed by Dr. Hagner's apparatus. It seems to do what I have been trying to do by a variety of methods, none of them satisfactory. The methods of suture which I have been trying were satisfactory, but unnecessarily prolong the operation, which is undesirable if it can be avoided. This bag seems to me to promise well; it should shorten the time of operation, cut out the frills and enable us to get through. I think it is an excellent device.

Dr. Gibson: I do not think I have got anything more to say about that little trick I brought to your attention.

The question of anæsthesia, of course, is one we are all interested in. I held off from the gas and oxygen for a long time on account of the prejudice of my particular anæsthetist, but little by little I began to use it more and more, and after a very illuminating visit at Cleveland and seeing the freedom with which it was used there, I have used it, employing anæsthetists who are skilled and like to give it.

It is perfectly wonderful what gas and oxygen will do for these old people. What Squier has said half jokingly, half seriously, I can repeat with all seriousness. These old men get half a grain of morphine and come to the operating room sleepy and dopy. The operation is done quickly, under ten minutes sometimes, and you withdraw the anæsthetic, and the old fellow looks up at the clock and says, "Why, it is only ten minutes ago that I was brought in here." What Squier said about their sitting up and reading the newspapers is borne out by a recent case. I operated on an old gentleman in the morning, and when I came back in the afternoon there he was sitting up and reading a newspaper, and what is more, he had written home telling his wife about the operation. There isn't much shock about that, psychic or otherwise.

As regards bleeding, the Lord has been very good to me. I have never seen but one serious hæmorrhage, in one of my early prostatectomies. I do believe we ought to save blood, and I am going to have one of these little bags ready when the psychological time comes to use it.

I rather lost my faith in spinal anæsthesia when Prof. Kustar came over here a few years ago to boom spinal anæsthesia and read a paper before the Surgical Association. I had a heart to heart talk with one of his German colleagues after the discussion and he said to me, "I can easily understand why you Americans stick to ether. We in Germany give anæsthetics so rotten that we have got to find some other method." I think there is a good deal in that. I have used spinal anæsthesia under certain indications, and will again.

As regards cases that die, I have had my mortality, a fair share of it. An instance of the mortality is a case in which I did not recognize the cause at the time, a very simple, easy prostatectomy. The patient never came out of his anæsthetic, and died within two or three hours after operation. It never occurred to me what the trouble was until I read an article some time afterwards on the question of air embolism. In those days I distended the bladder moderately with air, but I have given it up since that time. I think the cases we lose are the cases where our hand is forced. In private practice you can pick and choose your patients and can use your judgment as to whether the patients can worry along, but in a general hospital service we have a lot of stuff dumped on us, the cases are in the terminal stage, they have no funds, no resources, no friends, and they come to us ordinarily showing much constitutional disturbance. There is generally bleeding from trauma to the urethra, and we find it difficult or sometimes impossible to get into the bladder, and those patients are in a very disagreeable condition. It is there that the two-stage operation does good, and yet there are some pitfalls to the two-stage operation. These patients, some of them, are so ill that they could not stand that. On two occasions the two-stage operation was complicated by a very prolonged disturbance of wound healing, and we have practically healing by second intention. One of these patients that I did a two-stage operation on failed to respond at all, and constitutionally showed so many manifestations, his temperature did not come down, and finally I decided there was something that I did not understand, and I found on operating that as a result of false passages the prostate was the seat of multiple abscesses. That was a life-saving operation.

Dr. Hagner: I want to warn Dr. Gibson and Dr. Thorndike not to keep the bag too long; as it is well known that rubber doesn't keep indefinitely.

I have not used anything in my prostatic work except gas and oxygen for over two years; I have used novocain infiltration in the skin, but not in the prostate. The following incident shows how the anæsthetic may get the credit for causing an accident when it has nothing in the world to do with it. A man with severe hæmorrhage from the prostate was brought to Garfield Hospital; it was decided to do a cystotomy, the anæsthetist (to give nitrous oxide) was sent for. While waiting, the man had a stroke of apoplexy. If the man had been given the nitrous oxide it would have been called an accident from nitrous oxide anæsthe-

sia. I operated later and took the prostate out under nitrous oxide and oxygen.

The doctor spoke of the kidney as being of the utmost importance. I think he is pretty nearly right in that. The kidney is awfully important in determining the end result of these operations. I think I can back up everything he says from my own personal experience.

I do believe in the two-stage operation, especially in those cases he speaks of with large amounts of residual urine. It has been my experience that those cases, if you use plenty of irrigation, do not have this tendency to necrosis of the abdominal wound. The paper read by Dr. Caulk on the use of the Bulgarian bacillus will have an important bearing on the treatment of these cases, because it is just those cases that he spoke of that have this tendency to this necrosis of the wound.

As regards hæmorrhage, I would like you gentlemen to go home and ask your residents about your hæmorrhage. I would just like to cite something in Dr. Young's cases. In the punch operation of Dr. Young's I would like to say he comes here and tells us absolutely truthfully, as far as he knows I am sure, that those cases do not bleed. They do bleed. And all you have got to do is to go and ask the men who take care of them. Lots of Dr. Young's cases he does not see after the operation, and he doesn't know the trouble the residents have in getting the clots out. They are very fond of Dr. Young, and don't like to make him feel uncomfortable. I believe some of the residents of some of you gentlemen are exactly the same way. Mine are not so considerate of me. They tell me, or else call me up and I have to go out to the hospital at night and clean out the bladder.

If you use this little bag, and use it on a few cases, and then ask your residents what they think of it, I am sure everyone of them will tell you that it is a great comfort to them at least. I do not believe that all cases having hæmorrhage die, but just as Dr. Lower says, the loss of blood in these old men that have a low vitality, has a certain amount of bearing on the result in these cases.

Dr. Lower spoke of that patient in whom the pulse went up so quickly on account of hæmorrhage. That was really quite a remarkable case. When he was first examined he had fifty-eight ounces of residual urine. His phthalein output at the end of an hour was so small we could not estimate it. He had eight stones in his bladder, which was badly infected. The bladder was opened, the calculi removed and suprapubic drainage was kept up for three weeks. The phthalein gradually came up

until at the time of operation it was 24 per cent. As regards the effect of the hæmorrhage, his pulse was ninety when the operation was started. The man was quiet, and was taking nitrous oxide and oxygen. I never saw a case bleed so. Immediately his pulse went up, and as soon as the hæmorrhage was controlled it began to come down, four or five beats almost in a minute. In an hour his pulse was down to a hundred. It may not have been the hæmorrhage, it may have been shock, but it looked to me as if it was the hæmorrhage.

The blood pressure in these cases is very important. We must look out for those cases that have had a high blood pressure which has gone down, because they have a bad myocarditis. Because a man is suffering from low blood pressure is no reason he is not suffering from myocarditis, and it is the very worst time to operate when a patient is in this condition. It is the attention to the small details in this procedure that makes a success or a failure of the operation.

In regard to the infection of the space of Retzius, I think that one of the important steps in its prevention is to be careful when opening the bladder not to strip down too far over the front. Dr. Squier has brought this out. It is important to open these bladders high up. If they are opened low down they take much longer to heal.

As regards continuous irrigation, I think it is good. You take a leg ulcer, for instance, and put it in a continuous bath and it will begin to granulate, and I believe that this continuous irrigation kept up from twelve to twenty-four hours rather lessens the absorption that the patient gets from suppuration in the abdominal wound and the wound where the prostate has been removed. If the glass tube runs down far enough into the rubber tube, then the pressure of the muscles will not cause occlusion of the tube by compressing it.

Dr. Chute: In answer to Dr. Morton, I will say that I have not been troubled with sloughing and necrosis of the abdominal wound to any extent. I have had the fascia slough a little, but never seriously. I have had redness and induration of the wound, but no fatal sepsis. What I usually do is to put a little rubber wick in the space of Retzius and one above, and with this drainage the wounds do not seem to suppurate to any great extent.

In answer to Dr. Cabot's remarks in regard to the cardio-vascular system, I do think that in old men one must always watch the cardio-vascular system, but I do not think there is any reason to treat these prostatics any differently in that respect than other old men.

I do not do this two-stage operation in anything like all my cases, but it seems to me that in the bad ones it is a great help. I believe it is more or less of an insurance against embolism, as well as hæmorrhage. Open up the bladder in one of the bad cases and leave it for a time and I believe you diminish the liability to embolism through diminishing the congestion.

I do not agree with Dr. Hagner that we may take the resident's word as final as to how much hæmorrhage occurs in these cases. A relatively little blood makes a considerable showing when mixed with urine. I believe unless a man is accustomed to estimating this, it is very easy to be mistaken.

A CASE OF UNUSUAL PROSTATIC CARCINOMA, ORIGINATING APPARENTLY IN THE SUB-CERVICAL GLANDS

By James Pederson, M. D., of New York

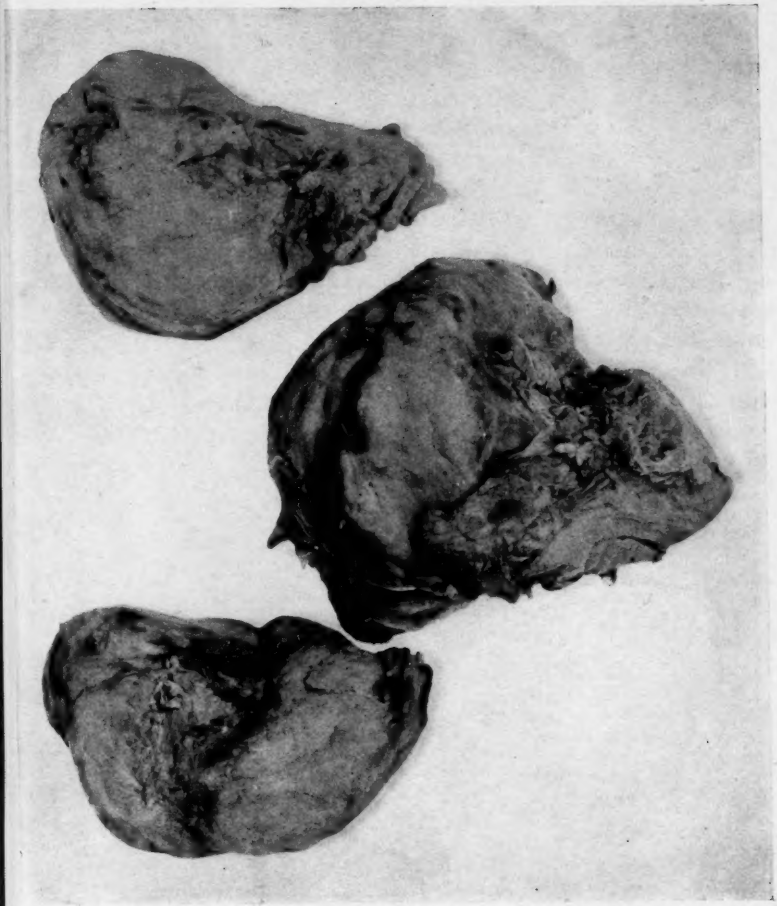
THE patient is forty-seven years of age, a farmer with good physique. Apparently his first bladder symptoms developed late in April, 1913. He first noticed hæmaturia, then frequency of urination; later occasional clots were passed.

Cystoscopy showed a large mass having an irregular outline presenting a fringed appearance with small cystic prominences here and there. Though no ulcerations were seen, the mass bled very easily. It extended across the base of the bladder and seemed to spring from the trigone which it hid completely.

Operation October 30th, 1913. Exposed through a transperitoneal cystotomy the mass presented two distinct and separate halves, symmetrically placed as to the median line of the bladder base and occupying the cavity of the bladder. Their rounded form, their almost equal size and the sulcus formed by their opposing inner surfaces, suggested at first glance an extreme and unusual intravesical enlargement of the prostate. On closer inspection, combined with palpation, it was found that the two pedicles sprang from two points right and left respectively of and close to the median line, above, i.e., proximal to the internal sphincter. Hence the supposition, that the growths originated in the group of glands beneath the mucosa of the vesical neck—the subcervical glands.

The pedicles were dissected out from their attachments; where possible the mucous membrane was brought together

PLATE XVII.—Presented by Dr. James Pedersen



over the raw surfaces thus made; the actual cautery was applied to the rest.

Convalescence was uneventful. The patient left the hospital on December 4th, with a slender suprapubic sinus that allowed a little urine to escape from time to time. Excepting frequency and some urgency, his urination was normal. The suprapubic sinus closed in the course of a few weeks. The bladder function gradually returned to nearly normal.

The pathological examination was made by Dr. Frederic E. Sondern, who reports as follows: "The specimen consisting of three large pieces of firm tissue, showing a firm connective tissue stroma with abundant inflammatory infiltrate of round cells and polynuclear leucocytes. Infiltrating the stroma are masses of epithelial cells which are generally large, but vary considerably in size. Diagnosis: Carcinoma. The tumor probably originated in the prostate, but it is impossible to determine this point absolutely."

DISCUSSION

Dr. J. Bentley Squier, of New York: The amount of prostate that could be enucleated and felt was practically nil. His prostate seemed to be simply a contracted fibrous ring. That was also cleaned out, which I think is the interesting part of the whole story, along with the pathological examination. It shows that if the tumor did spring from the prostate it must have developed from the sub-cervical glands or the glands of Albarran, and that it was not extravesical at all.

RADICAL SURGERY IN ADVANCED GENITAL TUBERCULOSIS

By George S. Whiteside, M. D., of Portland, Oregon

SIX years ago when I first reported on this procedure and four years ago when I had collected twelve cases, I found to my surprise, that it aroused much comment both favorable and unfavorable. [I stated at that time, and I still maintain, that it should be reserved for advanced cases; or I might better say selected advanced cases.] The papers published by many different surgeons in the past five years have led to a more thorough knowledge of the pathology of genital tuberculosis and have showed a striking unanimity of opinion in regard to the results obtained by conservative surgery in early cases. Everyone now seems convinced that genital tuberculosis oftenest begins in the epididymis, that the vas is early involved, the prostate and seminal vesicles later and the testicle itself latest of any part of this system. The bladder may or may not be infected. Everyone agrees that when the epididymis and vas are the starting point, and there are only small tuberculous nodules in the prostate, that epididymectomy, including removal of the vas far into the ring, is usually sufficient to prevent extension and aid the patient in overcoming the slight prostatic infection, so that the result seems to be permanent cure. In this connection I highly approve of leaving the proximal cut end of the vas draining through a button-hole slit in the skin above the incision in the groin. This manœuvre was, as far as I know, adapted by Cabot from Rovsing's well-known similar method of disposal of the cut end of a tuberculous ureter after nephrectomy. There is no question, no division of opinion as

to the best course to pursue in early or moderately early cases. It is only of late and apparently hopeless cases that I wish to speak today. In such no operation is too radical, no sacrifice of tissue or organs too considerable if by so doing the patient's life can be saved. I will go further and say that no mortality of operation short of 100 per cent is high enough to discourage such a last and only chance of saving life. If only one patient in 100 so operated on gets perfectly well, then the 99 failures are amply justified. In my former papers referred to I stated, and I still maintain that radical operation should be reserved for advanced cases —or I might better say selected advanced cases because in advanced genital tuberculosis without some such radical procedure death is slow but certain. I have, in all, 22 cases who have submitted to operation for removal of the whole or half of the genital tract. This means extirpation of testicle, epididymis, vas, seminal vesicle and prostate on one or both sides of the median line. In reviewing these cases and summing up results after several years I can say—

Absolutely cured	4 cases
Died from pulmonary phthisis	9 cases
Died within a few months from local infection	3 cases
Lost sight of, presumably they may be dead	6 cases
Immediate operative mortality	0 cases
Total	22 cases

I have besides 4 cases on which it is too early to report at this time. This gives almost 20 per cent of real recoveries. One of these four cases was the first and remains the best and most brilliantly successful one. Since 1909 I have modified my technic only in minor particulars. I have tried extirpation of prostate and vesicles from above as suggested to me by Watson, and although the patients were particularly favorable subjects I will not try it again that way. The vesicles are easily exposed from above but if, as often happens, suppuration takes place after operation the wound drains badly and absolute failure by death within a short time either from

peritoneal infection or from long continued and exhausting sepsis in a deep, poorly drained wound is apt to be the sequel.

Twice I have operated when known areas of pulmonary consolidation were present. It is a grave mistake to do so. Death is hastened and there is no chance of cure. Drainage should always be left both through the scrotal and through the perineal wound. Suppuration almost invariably occurs, in spite of every precaution, I think because of the poor resistance of an advanced tuberculous patient. Such adjuncts as fresh air, good food, tonics and tuberculin should be resorted to both before and after operation and continued for months or in some cases years. Some of my cases have had tuberculous involvement of the bladder and three of them of the bowel besides. These are not contra-indications to operation because after removal of the more important and extensive deposits in the genital tract the bladder or bowel will almost surely get well in course of time. I think there is little more to say. To sum up I believe because it offers the only hope of life to an otherwise doomed patient this very severe and drastic operation is justified. In 22 cases in the past six years I have had no immediate deaths and four eventual complete recoveries.

DISCUSSION

Dr. J. Dellinger Barney, of Boston: Genital tuberculosis is a subject in which I have long been interested, and it seems to me that our own experience has been somewhat different from that of Dr. Whiteside. We consider, from the experience that we have had in the early cases, that is, the cases of slight infection of prostate and vesicles, that they do not need operation, and the cases in which the infection is well advanced had better not be operated upon. We have followed pretty carefully all the cases that have had operation, either by orchidectomy or epididymectomy, and I do not recall in that list, which comprises something like 175 cases, but one which has eventually had a flare-up or more severe infection of the prostate than he had at the time of operation. On the other hand, these cases have improved to such an extent that, whereas at the time of operation the prostate was markedly tuberculous, after operation, six months, or a year, or even ten years when I have seen them, they have come in with prostate and vesicles very much different, almost normal and clear of the tuberculous process.

It seems to me that that argues against the operative measures which other men have been trying. We have perhaps not been seeing the kind of cases of which Dr. Whiteside speaks, but I feel certain that time, tuberculin and hygiene will do, in most cases, what other men seem to accomplish with the knife, and without the danger of setting up a persistent sinus in the perineum or of lighting up foci elsewhere.

It also might be stated that these cases of genital tuberculosis are not the slight localized affairs that they may seem to be. They are primary in the genital tract as we say, but of course they are secondary to tuberculosis elsewhere. Following these cases over a long period of years shows them in many instances to have developed other tuberculous foci in bone, lung or joint, or something of that sort, and in many death has ensued from tuberculous processes, even at a period of twenty years after the testicle was first removed. This shows that the tubercle bacillus is still in the body, and that the operation on the genital tract does not by any means quiet that process down.

Dr. R. F. O'Neil, of Boston: I should like to mention in connection with this discussion a case I have recently seen, a young Frenchman who six years ago had an orchidectomy done in France for tuberculosis. When I saw him he had a thickened nodular epididymis on the opposite

side with evidently an abscess at the lower pole. His urine was clear at that time, there was no urinary frequency and nothing to suggest any renal involvement. I did an epididymectomy on the remaining side to conserve the testicle for its internal secretion. He did well for a few months, and then his urine showed a little pus. I had a guinea-pig injected, which was negative. I saw him about a year later, and he then had considerable frequency and pyuria. I made a cystoscopic examination but was unable to catheterize the left ureter because of obstruction. The urine from the right kidney on two occasions showed pus and tubercle bacilli and was positive to the guinea-pig. I made a third attempt to catheterize the left side, but was again unsuccessful. The X-ray showed a very large, dead, caseous kidney on the left side. He now has tuberculosis of his remaining kidney. The phthalein output is in the neighborhood of 30 per cent. Following his epididymectomy I was very much surprised to find his renal outfit in such bad condition. The destruction of the occluded left kidney must have been very gradual and doubtless could have been discovered when I first saw him had there been any apparent reason to investigate the upper tract. Whether removal of the left kidney at that time would have prevented the infection of the right is of course purely speculative. There is no evidence at the present time that it is doing any harm.

Dr. James Pedersen, of New York: I think it was three years ago that I reported before the Association two cases of uro-genital tuberculosis treated along the lines recommended by Dr. Barney. One of those two cases I should like to refer to again.

The patient had a tuberculous epididymis. The diagnosis was made by exclusion, and the late William T. Bull advised orchiectomy, which was performed. The patient went along without any further symptoms of any kind for six years; then developed a marked cystitis without apparent cause. Cystoscopy and catheterization of the ureters revealed tubercle bacilli coming from the right kidney. The certainty of nephrectomy, and the uncertainty of tuberculin and hygiene were presented to him. He chose the latter, preferring to hold the operation in reserve. Fortunately, he was able to keep himself under ideal conditions. His family physician carried out the tuberculin treatment with great system and regularity. The patient recovered. By the end of approximately a year, the urine contained neither pus nor tubercle bacilli, and the patient was symptom free. That was five years ago. The patient is still in

good health, and has no subjective evidence of renal nor vesical tuberculosis.

Dr. John R. Caulk, of St. Louis: In view of the fact that most cases of genital tuberculosis are believed to start in the globus major of the epididymis, I will say that during the last year I have had three cases and all three have shown just the globus minor involved and the rest of the epididymis uninfected.

Dr. Keyes: Most of them do start in the globus minor.

Dr. Barney: All we have had began in the lower pole.

Dr. Hagner: It has been my experience that the globus major was involved in the majority of cases, unless it is a secondary involvement. The globus major is more frequently involved primarily than the globus minor. Every text-book will tell you that.

Dr. Caulk: These are the only cases I have had involving only the tail; all the others have started in the globus major. I was taught and have taught this as the important diagnostic point between gonorrhœal epididymitis and tuberculosis and that has been my surgical experience.

Dr. Edward L. Keyes, Jr., of New York: I want to make two protests; first, against the statement that a tuberculous kidney could spontaneously heal without complete destruction; and second, against the notion that genital tuberculosis usually starts in the globus major. I, too, in my youth cherished that delusion. I got it out of the text-books. I have, however, in looking over our own cases discovered that the great majority started in the globus minor. I have also in certainly four or five instances noted, sometimes bilaterally and sometimes on the first side involved, that only the globus minor was involved at the time of operation, so that in one case, that of a man about to be married, I did what perhaps was a fool operation; I did a bilateral hemi-epididymectomy, telling him that if all went well I might perhaps later anastomose his vas. I have never seen tuberculosis involve only the upper end at the time of operation, except in one case—which proved to be syphilitic!

Dr. Francis R. Hagner, of Washington: We know very well that in the majority of these cases of involvement of the testicle by the tubercle bacillus it either takes place through the lymphatics or through the blood, unless it is secondary to bladder tuberculosis or prostatic tuberculosis, in which case it would begin in the globus minor. The reason it occurs in the globus major is due to its coming through the blood.

DR. CAULK: Dr. Keyes made a suggestion that has impressed me during the last two years. I have been following these epididymal cases as a matter of differential diagnosis. We see in the colored race in St. Louis a great deal of syphilis. In fact, almost every negro in the clinic has syphilis. But we have been struck with the great frequency of nodular epididymes, without apparent involvement of the testicles, in syphilis, and it is often hard to differentiate between syphilis and tuberculosis without a very thorough study.

DR. KEYES: Did you operate on these cases?

DR. CAULK: No.

DR. KEYES: You will find the nodule in the testicle.

DR. CAULK: They seem to be in the epididymis.

DR. KEYES: They are always in the testicle.

DR. CAULK: The epididymis has seemed to me at least to be the most involved.

DR. WHITESIDE: I have been much edified by this discussion, but I feel that it did not have quite as much to do with the paper as it should have. In the cases that I am trying to talk about it does not make any difference whatever whether it began in the globus major or minor. The cases that I am trying to talk about are the ones where the mass in the scrotum is about as big as your fist, with usually two or three suppurating sinuses through the skin of the scrotum, where the vas deferens is as big as a cigarette and the prostate distinctly nodular, and the vesicles the same; where there is often either miliary tuberculosis or ulcers in the bladder, as can be seen on cystoscopy, and where, as far can be ascertained, the kidney has not become infected. Where the kidney has become involved or where the lung has become involved or where the bone or the hip-joint has become involved, then it seems to me that such a case is a hopeless one from the point of view of the operation I have proposed. Therefore I said in my paper that it was for *selected* cases of advanced tuberculosis and not every case of advanced tuberculosis. The kind of cases that I am talking about are cases that are so far advanced that hygienic methods are not going to cure them. They are going to die unless you do something for them, and it seems to me that it is only reasonable to give them any chance that they may have.

Dr. Barney said that the early cases did not need operation, and that advanced ones had better not be operated on, and yet he reports 175 cases that were operated on.

Dr. Barney: We operate on them every day by orchidectomy or epididymectomy, not by prostatectomy or vesiculectomy.

Dr. Whiteside: The early cases with no suppuration of the prostate and just a little knob as small as a filbert in the epididymis are, I grant you, much better treated by the method of Dr. Barney. That is not the class of cases that I am talking about. Undoubtedly such slight infections of the prostate do heal after epididymectomy, in a large number of cases, without further trouble, just as Dr. Barney has said.

I question whether genital tuberculosis is always secondary to tuberculosis in bone or lung. I think sometimes the tuberculosis in bone or lung is secondary to genital tuberculosis. As I said in the paper, in a great many cases of genital tuberculosis death results later, often many years later, from secondary tuberculosis of bone or lung or other organ.

Dr. O'Neil has reported an unsuccessful orchidectomy. He said that six years ago a patient had an orchidectomy. Now the other side was involved, but the urine was clear. Afterwards he had pus in the urine, but it was negative to the guinea-pig, and he had later than that frequency and pyuria, and then it was positive with the guinea-pig.

Dr. O'Neil: What I meant to illustrate was the advisability of mixing up with a tuberculous process at all; and that beyond a certain point the less you did the better off the patient was.

Dr. Whiteside: Of course, it would be a very nice thing if someone else would attend to all these cases. I have been much disappointed with guinea-pigs, both positive and negative. Many of them are tuberculous anyway, and sometimes they do not become tuberculous when they should.

These cases that I am speaking about are the advanced cases that nothing else can be done for. I am not talking about these early cases. I am not in any way infringing on the territory so ably covered by Dr. Barney and our Boston associates.